

THE MEDICAL NEWS.

A WEEKLY JOURNAL OF MEDICAL SCIENCE.

*VOL. XLV.

SATURDAY, SEPTEMBER 27, 1884.

No. 13.

ORIGINAL LECTURES.

ABDOMINAL SECTION IN DISEASE OF THE UTERUS, OR OF THE UTERINE APPENDAGES.

A Lecture delivered, by invitation, at the Jefferson Medical College Hospital, September 15, 1884.

BY LAWSON TAIT, F.R.C.S.,

SURGEON TO THE BIRMINGHAM AND MIDLAND HOSPITAL FOR WOMEN, BIRMINGHAM, ENGLAND.

GENTLEMEN: I really do not know what Dr. Parvin has brought me here for, unless it be that there is a tendency in your country, to a much greater extent than in my own, to overwhelm your visitors with kindness of every description. I have been constantly told since my arrival in this country, that I have come to the country of big things. I certainly believe that I am now in the city of biggest things in the country of big things. I have to-day seen the biggest store, and have eaten the biggest oyster that I have ever seen. I am told that this is the biggest medical amphitheatre in the world, and it certainly is larger than any in England. Now I am asked to do the biggest thing of all, to talk about patients whom I never saw until this afternoon and then only for a few minutes. This is a very big thing, and I am afraid that I shall fail in the attempt. If I should make mistakes in diagnosis, please understand that it is because I have caught the American disease of hurrying through everything at a rate which none but an American can keep up.

You very well know, I am sure, that one of the most obscure things that a man can attempt in the shape of diagnosis, is the diagnosis of disease inside of the belly. An illustration which I frequently use with my patients is this. I say to them: "This is a table. There is a cover on it. I can tell you that it is a table, but until the cover is removed, I cannot say of what kind of wood the table is made." The man who ventures on accurate diagnosis in abdominal disease is just the man to get into trouble. There is no region of the body where diagnosis requires more caution than in that of the abdomen. One reason of this is that any mistake will almost certainly be discovered. As a distinguished American surgeon said to me a short time ago, we are not so well off as the physician. He hammers on the chest, finds certain abnormal sounds and says that such a condition is present. He may be mistaken, but the patient probably moves away and the error in diagnosis is never discovered, but in abdominal surgery, the surgeon after giving his opinion usually at once proceeds to the operation, and any mistake is almost certain to be discovered.

A series of patients are coming in, and my friend, Dr. Parvin, has asked me to make some remarks on them.

OVARIAN TUMOR.

Here is a patient who, as far as I can see, is the victim of a disease which is very common with us, and I

suppose as common with you. At first sight, it looks like an ovarian tumor. The first thing which attracts my attention is a scar from a puncture, and here I see the remains of another puncture of an older date. I next notice the uniform shape of this abdomen. There is a symmetrical uniformity about this abdomen which is suspicious. When you see a perfectly uniform enlargement of the abdomen, begin by suspecting that it is not due to an ovarian tumor. The chances in such a case are greatly in favor of one of three things. In the first place, pregnancy, which you must always eliminate; in the second place, a small tumor with malignant growth and ascitic effusion, which is the most likely of the three; and, in the third place, the presence of a parovarian tumor.

I next place my hand on the tumor,—and here let me give a caution. When you are dealing with abdominal disease either for the purpose of diagnosis or treatment, you cannot be too gentle in your manipulations. If at all rough in your manipulations, the first thing you do is to frighten the patient and obscure the diagnosis. The abdominal muscles will be contracted, and you will not be able to learn a great many things which it is desirable that you should learn. If in treating abdominal disease you handle the parts roughly, you run a risk of doing harm. I touch the abdomen gently, and I have already learned a good many things. I learn, in the first place, that this certainly is not pregnancy, although I knew that before. I learn, in the second place, that it is not a parovarian tumor. I learn, in the third place, that it is probably a small tumor with a large amount of ascitic effusion.

I feel in the lower part of the abdomen a semi-solid mass, and above this a mass which is not solid. Our business is to determine what relation the mass not solid bears to the mass which is solid. Above, we obtain on percussion the resonance of the intestine. There is a matter here which obscures the diagnosis. That is the fact that she has been tapped. I get an intestinal note above, and there is evident fluctuation, but from these two factors I cannot positively determine which one of two conditions is present, and it is a rather important thing to know which we have before giving advice.

The two conditions to which I refer are the following: This may be a large cyst which has been emptied by tapping, or it may be merely ascitic fluid. If it is a large cyst which has been partially emptied, or which, having been emptied, has become partially refilled, it is a case of multicystic cystoma, which can be dealt with in a satisfactory manner. In the second place, it may be a small cyst covered with a large effusion of ascitic fluid. If this be the case, it will be necessary to engage in the discussion of a number of points before making up our mind.

I have looked at the patient's face but I find nothing there to guide me. I have examined the pelvis, but I find there nothing but negative indications. The uterus is small and tolerably free. On the left side there is a

small tumor which may be one of two things, either the left ovary in a state of incipient enlargement, or a small mass of papilloma. This may be a single ovarian tumor and the condition here may be the result of malignant proliferation on the outside of the tumor, or on the parietal peritoneum, or the peritoneal coat of the viscera. It is important to know which of these two is the more likely. With a half-full abdomen like this, one cannot pretend to give an opinion. The fluid has been removed and reaccumulation is taking place. Although it is impossible to give a positive opinion, I have a suspicion that the fluid which was removed was not removed from a cyst. There is a small tumor in the lower part of the abdomen, and I think that the fluid which was removed was ascitic and that there is here a condition of papilloma.

Suppose it is impossible to come to an exact conclusion, what ought to be done? Open the abdomen in either case; for, unless you are absolutely certain that the disease is incurable, it is, in my judgment, a surgical crime to allow a patient to go to the grave with an abdominal tumor, without an effort being made to save her. This should be done even when papilloma, which is a most unfavorable condition, is suspected. (The patient was now removed.)

For reasons which are obvious, it is better to speak in the absence of the patient. I do not know who is responsible for the tapping in this case, and it does not much matter, for the practice of tapping is still largely carried out in my own country, and I presume the same is the case in this country. Therefore I never very much blame a man for tapping in these cases. My opinion, which coincides with that of most operators, is that a woman with abdominal disease, unless the affection is clearly malignant, should not be tapped. This rule should be adopted for many reasons, to some of which I shall refer. In the first place, speaking from my own experience in the removal of ovarian tumors, I can say this: that for a number of years (five or six) I have not lost a case of ovariectomy which had not previously been tapped. It is perfectly certain that tapping increases the risks of ovariectomy to a much larger extent than anything else. There is no kind of adhesion, there is no kind of complication, there is no kind or size of pedicle, which makes any difference. The worst cases recover as well as the simplest, provided they have not been tapped. I cannot tell why this is, although I suspect that there is a chemical reason for it. The chemical analysis of the contents of ovarian tumors has been abundantly discussed, but as yet no satisfactory results have been obtained; but it has been found that the materials removed from the interior of ovarian cystomata are extremely rich in some mysterious forms of albumen. I have carefully examined the fluid contents of these tumors by various elaborate methods, and I am of the opinion that the albuminous elements of no two ovarian tumors are exactly alike. If this peculiar albuminoid substance is removed to a large extent, the blood is deprived of something which renders the other ingredients more coagulable. The patients who have died after ovariectomy, and who have been previously tapped, have died from the formation of a clot starting in the pedicle and reaching to the cavities of the heart. In this fact we have one indication that tapping should not be performed except under exceptionable circumstances. The

certainly with which ovarian tumors come under notice at an early stage, and the ease with which the diagnosis is usually made, render it important that the rule that tapping should not be performed should be recognized. It is the duty of every practitioner of medicine, when he sees a case which he suspects to be one of ovarian tumor, either at once to proceed to its removal, or at once to place the patient under the care of some one who will do justice to the patient and to the art of surgery which he represents. I say that as soon as an ovarian tumor is recognized, you should refrain altogether from tapping, and immediately remove the tumor.

The patient whom we have had before us has been tapped. I do not know whether the fluid removed was ascitic or from a large cyst. My suspicion is, as I have already said, that the fluid was ascitic. Among other things which lead to this opinion is the position of the punctures, which is above the point at which tapping of an ovarian tumor usually takes place. The surgeon who tapped this woman no doubt felt this solid mass at the bottom of the abdomen, and probably felt more certain of obtaining fluid at the points where the punctures were made than lower down. This is presuming that you elect to tap at the same point that we do, *i. e.*, midway between the umbilicus and the pubes.

At this point some critics might ask "What do you make of those cases in which tapping was done over and over under the old practice, and sometimes under the new, for some patients will not submit to the radical operation?" In regard to the latter point, there is no difficulty with that now. During the last five or six years I have not had a patient come to me with an ovarian tumor, who has refused to have it removed. I can assure her that the chances are 98 out of 100 that she will get well, no matter what the age, no matter what the appearance of the tumor, and no matter what complication may be present, provided it is not malignant disease and that there has been no previous tapping. What became of those cases which were tapped so often? Recently I discovered on a tombstone in the south of England, an inscription stating that the person buried there had been tapped for ovarian dropsy forty-seven times. That is the largest number of tapings that I know of, and of course, being on a tombstone, the statement is certainly true. My answer to the question proposed is that these were not cases of ovarian tumor. They were tumors of the parovarium. The facts concerning these apparent exceptions, are really in favor of the view which I have expressed in regard to the tapping of ovarian tumors. The contents of pariovarian tumors contain little or no albumen. These cysts are generally filled with a limpid fluid consisting of simple water with a few salts. Sixty or seventy per cent. of the parovarian tumors which I have examined have contained no albumen. If you get hold of a large cyst secreting a non-albuminous fluid, and remove that fluid repeatedly, the removal of the fluid will not drain the system much, for it is an easy matter to replace water with a small proportion of salts. This apparent exception therefore proves to be an argument in favor of the view which the writings of Dr. Bantock and myself have been largely instrumental in bringing before the profession that tapping is not to be allowed.

Suppose you get an ovarian tumor, when should it be removed? The arguments are all in favor of early

operation. The patient is not distressed with the suffering entailed by carrying around a large mass; she is not subjected to the likelihood of the development of the condition of papilloma which we suspect in this case; she is not subjected to the anxiety and worry, especially if unmarried, which her appearance will always cause, and the incision will be shorter than when the abdomen is large. The mortality of early operations is almost *nil*. If the tumor is removed before adhesions form or other complications occur, I believe that the mortality would be absolutely *nil*. My own experience leads me to believe that if the practice were uniform all over the world of removing ovarian tumors as soon as discovered, the mortality would not be one per cent. This is a matter on which, even in England, there is a good deal of difference of opinion. When you find an opinion expressed adverse to the one which I have just stated, you will find that it is expressed by one whose hair is white. I do not mean by this to say that young men are always right, or to imply that old men are often wrong. We have a saying in England that "young men think old men to be fools, but old men know that young men are fools," but a qualification of this exists in the fact that the world moves on, and as one generation succeeds another, the wisdom of those who have gone before is added to by those who are now at work.

I shall not now enter into the various steps of the operation, and indeed without a display of the procedure it would be almost a waste of time to do so.

Suppose that we were certain that this patient was suffering from papilloma, that the disease of which we are so much afraid was developing around the tumor; even if I were certain that such was the case, and I were responsible for the treatment of this patient, I should proceed to the removal of the tumor. The reason for that is a very curious one, and one which I cannot pretend to explain, but of the facts of which I am quite certain. I cannot say, without referring to my case-books, how many ovarian tumors I have removed, but in a considerable percentage both of parovarian and ovarian tumors, and also of cases of myoma, and also in cases where there has been no tumor at all, I have opened the abdomen, sometimes knowing what I should find and at other times not knowing, and I have found this curious velvety, warty condition of the peritoneum. One of the most extraordinary cases which I have ever met with, was one sent to me by Mr. Oliver Pemberton, of Birmingham, whose name is probably familiar to many of you. In this case there was great enlargement of the abdomen, supposed by several who had examined her to be a parovarian tumor. As soon as I placed my hands upon the abdomen, I was certain that there was no tumor, but simply an enormous effusion of ascitic fluid. In such cases as this I never tap, I always make an opening in the abdominal wall large enough to admit the introduction of two fingers, and thus obtain an intelligent idea of the condition of the abdomen, which cannot be obtained by gazing at the fluid falling from the end of a canula. There is no more danger in this than in tapping. So far as my own practice is concerned, tapping is absolutely discarded. In the case to which I have referred, I made the abdominal opening, and slipped in two fingers, and at once found that I had to deal with universal papilloma

of the peritoneum. I inserted a drainage-tube, and allowed it to remain two or three weeks, and completely cured the patient. She is now in robust health some four years after the operation. In another case, in a woman fifty-seven years of age, I removed a large ovarian tumor. Large masses of papilloma were also found. Two of these, each being larger than the fist, could not be removed, and after the operation could be distinctly felt through the abdominal wall. She is now sixty-five years old, in good health, and the tumors have disappeared.

It is certain that there are two kinds of papilloma, one of which is malignant, and which will kill the patient in a few weeks or months, and another kind which is not malignant, and which can be cured by removing the tumor or by opening and draining the cavity. I have submitted pieces of papilloma, some of which were obtained from cases which had been cured, while others had come from cases rapidly fatal, to the most experienced microscopists, and they have been unable to detect any difference between the two varieties. This curious condition, presenting as it does such extremely different features, so far as results are concerned, offers a very favorable field for careful research by pathologists. In this case, even if I knew positively that there was present an ovarian tumor complicated with ascitic fluid and large papilloma, I should still urge that if it is possible to remove the tumor, it should be done, for there is a chance that the patient will be cured.

I just now used the expression "if it is possible to remove the tumor," and that reminds me that I want to speak of one other matter in this connection. When you make an exploratory incision, you must carefully consider what you are going to do. You must prospect (I believe that is the word you use in this country) very carefully. You must not begin the operation for the removal of an abdominal tumor unless you are going to finish it. The most deadly things are operations which are half done. If a part of the mass is removed, and it is then found impossible to complete the operation, the chances are seventy out of a hundred that the patient will die. After I start an operation, I finish it, if it is in the power of a human being to do so; and this, I believe, is the point where experience tells in this special kind of work. Do not begin unless you are confident that you can complete the operation. If you have the courage of your convictions, and the experience necessary to do a difficult piece of work, go ahead and finish it. The chances of recovery will be infinitely greater than if you leave the thing half done.

REMOVAL OF THE UTERINE APPENDAGES.

The next case is one which would involve a great deal of talking, and one of which I cannot speak anything like exactly, for that would involve an intimate knowledge of the past history of the patient. For the purposes of instruction, however, I may assume what is doubtless the fact, that in this girl the sufferings are real and intense, and that everything short of surgical interference has been employed. I might with advantage talk of a case which I treated in the State of New York, in which the condition was to some extent similar to that of the present case, and in which the history was more completely known. For that matter, a supposititious case

might be discussed, for it would be easy to introduce into it those questions which are worthy of notice. This is all the more advisable because we have the tracks very well cleared in abdominal surgery on almost all points which are under discussion with the exception of one. The patient who has just been admitted to me comes under that category.

She is twenty-one years of age and has a pronounced crop of acne all over her face. When a woman enters my consulting-room, and I see acne, I always ask if she has been taking bromide of potassium. This is the fashionable drug for every conceivable uterine ailment, and yet I have never heard of any one who was willing to swear that he had ever cured anything with bromide of potassium which was worth curing. Still it is the one pump-handle which we have, and we work it pretty hard. When a woman comes to me with a yellowish-purplish face, covered with acne, I always ask if she has been taking bromide, and as a rule she says that she has taken large quantities of it; and, looking over her prescriptions, I find that she has been taking bromide of potassium, bromide of sodium, bromide of ammonium, and the last one I have heard of is bromide of nickel. These women wander from one physician to another. She, after a time, falls into the hands of one who puts in a pessary—for what, goodness knows. I saw a lady not many years ago from whom I removed both Fallopian tubes filled with pus, which had been there clearly from a miscarriage which she had had many years previously. Among other ways, she had been treated largely with pessaries. She had a large tray filled with pessaries, which she kept as a sort of curiosity. She had each pessary labelled; and she would pick up one and say, "This was given to me by Dr. A. It hurt me a good deal, but I bore it for some months." Taking up another, "This I got from Dr. B. It hurt me so much that I could not wear it long." Taking up a third, "Well, I believe that this one must have been invented by the devil himself. I could not wear it at all." These patients wander about from one specialist to another, getting from each one his pet pessary. Finally they go to a surgeon, who dilates the cervix with some contrivance, or he will divide the cervix in one way or another without giving any benefit. Then, after going for a long time from one doctor to another, she at last goes to a surgeon who has a reputation for performing operations. I do not mean to say that all these women who wander from one consulting-room or one hospital to another should be submitted to operation. I have never said this and never carried it out in my practice. What I do say is this, there are a large number of these patients which can be cured only by a surgical operation. Because at the outset of this new kind of work some mistakes are made, we are not therefore to close the doors against suffering women. Given the history of a woman who has wandered about as I have described in a somewhat exaggerated form, but none the less true, who has been subjected to every conceivable method of treatment, and she comes to you, and you are persuaded that her sufferings are real and not imaginary, you are justified, in my opinion, in making a small incision in the front of the abdomen to examine the belly, and to pass the fingers down into the pelvis to see if there is anything wrong there.

The first question is clearly, How do you recognize

the fact that the patient's sufferings are real? I cannot answer that question. All that I can say is, that never in my experience have I had a woman submit to the operation without finding sufficient cause to justify it. Of course, I, as all ought to do, place my statement and views, with what I propose to do and the results of the operation, immediate and prospective, clearly before the patient, and, as I say I have never known a woman to submit herself to the operation without finding sufficient cause to justify its being done. You say that this puts the responsibility on the patient. Well, that is what we do in every case. The patient cannot be relieved of all responsibility. A man comes to you with a diseased knee-joint. You lay before him the advantages and disadvantages of excision and of amputation, and then you ask him, "Will you have your limb amputated, or will you run the risks of excision?" There are few cases in which we can relieve the patient of all responsibility, and in these doubtful cases the patient must take a large share of the responsibility. In these cases, I always say: "Let us have a complete consultation with any one, or any number of your previous attendants. I will not take the responsibility of this operation without consultation with some one who is as responsible as I am." Having placed the matter thus before the patient, she must take her share of the responsibility.

It is perfectly impossible to imagine that in these cases there is any ground for the criticism which has been made, that women are having their uterine appendages removed for improper purposes. It would be impossible for this to be done without the matter being discovered. The proposal to remove the uterine appendages in order to prevent pregnancy has been made to me by women, but surely we have some kind of conscience. I do not suppose that because I have obtained a position of some eminence in this kind of work that I have been robbed of the conscience which I once possessed. When I say an operation is to be performed, it is done in a sort of semi-public manner. So far as I know, in English surgery, this charge has never been substantiated—indeed, it is never publicly made, it is only hinted at behind our backs.

Again, we are told that these operations are sometimes done for another kind of immoral purpose, *i.e.*, for the purpose of obtaining the fee which comes at the end of the proceeding. Well, this is rather a dangerous argument to use, because, if it were used against myself, I should at once publish a list of the men who had given bromides, used pessaries, and divided the cervix and ask how about their treatment in regard to the matter of fees. I do not know that they would come off as well as I should.

To return to our patient. This girl is twenty-one years of age; she has to make her own living, and this is a very important matter, indeed. If a woman comes to you whose husband has a large income, or whose friends are wealthy, the case presents altogether a different aspect. To the rich, luxury always contributes largely to the relief of pain. If a woman, whose husband has ten thousand a year, has a chronic inflammation of the ovaries, she will suffer far less than a woman who has to make her living and has the same disease. If a woman comes stating that for one week out of every four she is unable to work, you are bound to perform an operation for her relief. This girl has gone through

a long course of treatment. She suffers at her periods, but at other times is tolerably well. The indications for treatment are clear. If a woman tells you that there is one week out of every four that she cannot work, it is clear that the arrest of menstruation will afford relief.

As far as I can judge from the history of this patient, the operation which has been suggested is justifiable. You perform the operation, and what do you find? I have always found disease of the uterus or uterine appendages of some kind. These diseases are far more numerous than many imagine, and it would take a long series of lectures to discuss them thoroughly. On the left side, in this girl, there is a feeling as though there was a mass. I think that, in all probability, it would be found that the ovaries, like the uterus, are infantile in size and probably adherent. Suppose, however, that the appendages turn out to be absolutely healthy: I should still say that the operation was capable of being justified by the history of the case.

Now as regards the consequences of this operation; and these are the immediate, that is to say the risks of the operation, and the future consequences which the operation will involve. Of the immediate risk of the operation, I need not now speak. The future aspects of her life demand a little more attention. In the early days of vaccination, I think about the year 1792, a dean of St. Paul's preached a sermon against vaccination, arguing that vaccination was a terrible thing, because it tended to induce the growth of horns upon the head and cloven hoofs. We have been told that the removal of the uterine appendages induces certain changes, bringing on a masculine appearance, and other changes which are entirely theoretical. This mistake arises from applying to these cases the facts as to the results of emasculation of males and females before the age of puberty. It is perfectly well known from the practice in eastern countries, where the sexual organs are removed before the age of puberty, that such operations do result in the retention in the individual of the youthful conditions. The eunuch on whom the operation is performed before the age of puberty, retains his youthful appearance through life. Of course, this is easily understood, but it is entirely different when the operation is performed after the changes of puberty have taken place. Having performed the operation on a large number of women, I have never seen any alteration at all except that involved in improvement of health. When a woman is worn down with pain, loss of blood, and drugs, and you stop that drain, there is immediately noticed an alteration in the appearance for the better. Such statements as are found in the first edition of Sir Spencer Wells's work, and the still more exaggerated statements contained in the second edition of his work, are entirely without foundation.

What are the results? In the great majority of cases there is an immediate relief from suffering and loss of blood. In some cases the relief does not come immediately; but after a time, in a few cases, relief may not come at all; but this is no argument against the operation, any more than it is against many other operations. Take the operation of cataract. This is not always a success. It is probable that in about ten per cent. of all operations for cataract, suppuration of the globe takes place, and the result may rank as a mortality. In other cases escape of the vitreous or some damage to another

structure will result in such chronic inflammatory change as to leave the consequential results of the operation so bad that it may be classed as a complete failure. There is no realm of surgery out of which I could not pick abundant illustrations to show that in no other branch is success any greater, if as great, as in that of which I have spoken. Immediately after the operation the patient suffers from the climacteric; but this is inevitable in the life-history of every woman who lives to the age of fifty-two. I do not think that these women, who go through these troubles early in life, suffer any more, or even as much, as those in whom it comes at the natural time. Some do not suffer much, while others suffer a great deal.

So far we have not had any trouble, except from one thing, and this is a distressing one. It occurs after all sorts of abdominal operations, after exploratory incisions, after the removal of one ovary for cystoma, after the removal of both ovaries for cystoma, and after hysterectomy. I refer to the occurrence of acute melancholia. All the cases of mental alienation that I have seen following these operations are seven in number, and all have taken the direction of this most unfavorable form of insanity—acute melancholia. I cannot say that any one of them is likely to recover. I do not know that this is a necessary result in a certain number of cases. I have performed abdominal section some 960 times, and in this number I have met with 7 cases of acute melancholia. Of course, a good many of these cases died, especially in the earlier years of my practice. We may state that acute melancholia occurs in about one per cent. of those submitted to abdominal section. I do not know that anything like this follows other surgical operations. This is the only after-result of an objectionable character with which I am acquainted.

MYOMA OF THE UTERUS.

The next subject which Dr. Parvin has submitted for consideration is that of myoma of the uterus. There are two patients outside, but I do not think that it is necessary to bring them in, for you cannot see anything, and you cannot feel anything. I have examined the patients in the waiting-room.

One woman is forty-eight years of age, and does not suffer much from hemorrhage or very much in any way. The tumor is hard, shrivelled, and solid, and thus it is placed in the category of cases in which nature has cured the disease. In all probability, nature will not remove the tumor, but nature has so relieved the symptoms and so diminished the size of the tumor by shrinkage that nothing more will be required.

The other patient is forty years of age. She has had only two hemorrhages, and it is very likely that she can be tided over the climacteric without any surgical interference. Usually, we do not operate on women for fibroma after the age of forty-six or forty-seven unless it is perfectly clear that the use of ergot combined with absolute rest is insufficient to tide her over the climacteric. When, however, the disease appears in young women, say from thirty-five to forty, or as I have seen it in a girl of nineteen, an important question comes up for careful discussion, and here again the patient must accept a good deal of responsibility in the answer. If a patient spends one week of every month in bleeding and suffering pain, becoming anæmic, restless, and irritable,

unable to look after her affairs, and you cannot relieve the sufferings or arrest the hemorrhage except by operation, then this question must be considered. Is it worth while for that patient to go on suffering for a series of years when by an operation, the mortality of which is only four or five per cent., she could be relieved? On this point different men will express different opinions. If I were the patient, I should have the operation done. Holding that opinion, I advise the patient to have the operation performed.

Concerning myoma of the uterus, we have a number of traditions which are being rapidly destroyed. One tradition is that myoma is not a serious thing. We have been in the habit of finding, at our post-mortem examinations, a large number of myomata which have never given any trouble, but I need not say that the tumors which do not give rise to trouble, are not the ones which trouble us. The tumors which cause trouble are the ones which we see. If a tumor gives rise to hemorrhage and pain, the woman consults a physician, who recognizes its presence.

There is another tradition, that the occurrence of the climacteric arrests the growth of uterine myomata. It is now perfectly clear that a certain class of uterine myoma arrests the progress of the climacteric. Frequently we find women going on for years after the usual time of the climacteric, without any appearance of diminution in the size of the tumor, or in the amount of the hemorrhage. There is a peculiar kind of uterine myoma which causes but little pain or hemorrhage, but which goes on indefinitely increasing in size, and seems to be unaffected by the climacteric.

In uterine myoma, provided the use of ergot and rest does not give relief, one of two procedures may be adopted. The uterine appendages may be removed and menstruation, which seems to be the immediate process by which the growth is encouraged, arrested. It is a fact established beyond discussion, that in the great majority of cases operated on hemorrhage is immediately arrested, and the tumor shrivels up, and may disappear. The removal of the uterine appendages is an operation to be recommended in a certain class of cases.

In some cases in which the disease is not arrested by the removal of the uterine appendages, there is the far more dangerous operation of removal of the entire uterus, or hysterectomy.

I have now come to the end of the category of cases about which Dr. Parvin asked me to speak, and I have occupied more time than is usually allotted to clinical lectures. I am afraid that I may not have made myself perfectly clear, for I have absolutely no experience in teaching. I never gave a clinical lecture in my life until last week, when I addressed an audience similar to this in Albany. It is a misfortune that, in England, those of us engaged entirely in special practice never have any opportunity for teaching. The teaching seems to fall chiefly into the hands of those not busily engaged in practice, while those actively engaged in their profession are removed from all opportunity of teaching. I hope, therefore, that you will attribute any lapses in the sequence of my illustrations, or want of clearness in my narrative, entirely to my lack of experience, and accept my appearance before you, not as that of a British teacher, but purely as that of a British surgeon.

ORIGINAL ARTICLES.

NOTES ON CHLORINE AND ITS COMBINATIONS.

BY RICHARD MCSHERRY, M.D.,

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE IN THE
UNIVERSITY OF MARYLAND.

It happened recently that I was using chlorine in the form of chloride of lime, and otherwise upon a portion of my premises where such agents are often needed, especially in warm weather. About the same time I was prescribing it in two cases of diphtheria, not in the form of chloride or bichloride of mercury, the latter of which has been in common use with me for ten years or more in diphtheria, but in the form of Labarraque's solution directed as follows:

R.—Liq. sod. chlorinat,
Spt. vin. rectif., aa ʒj.—M.
S.—Twenty drops every hour in water while awake.

This was the principal though not the only agency, and it acted like a charm. I never used a combination that pleased me more. Almost coincidentally I received the June number of the *Sanitarian*, which contained a very interesting article from the pen of Dr. John G. Johnson, of Brooklyn, on "Canned Goods," which presented chlorine in combination, not as a disinfectant or a therapeutical, but as a poisonous agent.

Here was chlorine brought under observation acting efficiently in three different modes, to wit: 1, as a disinfectant and deodorizer; 2, as a valuable therapeutic agent in a most dangerous malady; and, 3, as a dangerous and insidious poison.

Not unnaturally I began to make a mental survey of a medicinal agent whose potentialities, in some form, I have been using almost daily for, certainly, over two score of years. Its profusion in nature is an indication of its great usefulness.

Considered as a disinfectant and deodorizer, all authorities and all practice put it in the front rank. It gained its repute before the term *germicide* came into common use, or, indeed, into any use. Thus, says Pareira: "As a fumigating agent, disinfectant, and antiseptic, chlorine, I believe, stands unrivalled. Halle, in 1785, appears to have been the first person who employed it as a disinfectant; but we are greatly indebted to Guyton-Morveau for the zeal and energy he manifested in his attempts to introduce it into use. For destroying miasmata and putrid odors, it is the most powerful agent known, and is, therefore, well adapted for disinfecting prisons, ships, hospitals, dissecting-rooms and other places the air of which requires purification."

A century, almost complete, confirms the opinion and practice of Halle. According to our modern American authorities, as Wood and Bache, Stillé and Maisch, chlorine is stimulant and disinfectant. According to Bartholow, chlorine gas will arrest putrefaction and decomposition of animal matters: "As a deodorant and disinfectant it may be used to destroy *foul effluvia and disease germs*." This is the eminent property of all the chlorinated solutions, possibly

through the influence of liberated ozone, but certainly with such influence, whatever the *ratio operandi*.

It is thus, therefore, that we have so many antiseptic, disinfectant, and deodorizing preparations of chlorine, regular and proprietary. Our chlorine gas, chlorine waters, chlorides of lime, of soda, of zinc, of lead, etc., or many combined, are so many testimonials to its utility. I have used, and still use, with satisfaction a solution of chloride of lead prepared according to an old formula as follows: Take of nitrate of lead $\frac{1}{2}$ drachm in a pint of boiling water, of chloride of sodium 2 drachms in a bucketful of water, when mixed, a sediment precipitates, and the supernatant liquor is a solution of chloride of lead, which is easy of preparation and efficient in use.

What are disinfectants? Halle never heard of germicides, but he had used them notwithstanding, and now, it seems, the two words are getting to be convertible terms.

Unless there is a great error in the progress of modern pathology, we may reasonably believe that the morbid poison of infectious diseases is commonly associated with the various microorganisms which are ever present in such diseases.

A disinfectant then is otherwise a germicide, and the most efficient disinfectants are the potent germicides, among which chlorine, in some form or combination, stands deservedly first.

The most accurate observers, foreign and American, as *e. g.*, Koch and Sternberg, place corrosive sublimate (mercuric chloride) at the head and front of the germicides. Turpentine, iodine in alcohol, carbolic acid, and creasote, are vastly less efficient agents. Dr. R. F. Weir, of New York, quoting Koch, says: "After applying a number of tests, similar to those employed in connection with a number of so-called antiseptics, we found that simply moistening the anthrax spores with a solution of one part of corrosive sublimate to 5000 of water destroyed them immediately. He then says that the sublimate is the only known disinfectant which succeeds by a single application of a few minims of a solution of sublimate, of one part to 1000, in destroying most resistant microorganisms."

In Dr. Sternberg's instructive paper on the "Germicide Value of Therapeutic Agents,"¹ we find, *inter alia*, the comparative quantities of several as necessary to destroy vitality or to prevent development, of which three may be offered from the most effective:

REAGENT.	Percentage required to destroy vitality.	Percentage capable of preventing development.
Mercuric bichloride, . . .	0.005	0.003
Carbolic acid, . . .	0.8	0.02
Alcohol, . . .	0.40-95	0.20-10

The presentation of these respective powers is suggestive.

It must be conceded that no germicide or disinfectant at present in use is equal to chlorine combined with mercury, or the mercuric chloride; but in the simple gaseous form, as when liberated from chloride of lime, which consists of quicklime to which chlorine adheres, hypophosphite of lime, and

chloride of calcium, it is of immense, indeed, of incalculable utility in therapeutic uses.

In passing from the germicide or disinfectant uses of chlorine and its combinations to the therapeutic uses, we are at once struck with its remarkable potencies. As it was used for germicide purposes before the term was known, so it was used for therapeutic purposes upon an erroneous theory, but with not less remedial influence. Thus calomel, a protochloride, was given as a cholagogue, with assured conviction that it acted upon the liver more surely and more effectually than any other medicine. When it was shown by the experiments of Hughes Bennett and others that calomel acted but little, if at all, upon the liver directly, physicians stood aghast, and soon agreed, to a great extent, that they had given the agent under a delusion, and many, better informed as they supposed, abandoned, and even ridiculed its further use. Nevertheless, calomel, cholagogue or no cholagogue, had remarkable therapeutic properties, and its use (not its abuse), in instances beyond number, had given the therapeutic results expected and desired by the practitioner. As it causes free evacuations of bile, it is in that sense, at least, surely cholagogue, but doubtless one of its effective properties is that of being destructive to low organisms. In some recent experiments by Hoppe-Seyler, and Wassilief, it was shown that albumen and blood mingled with calomel could be kept for days without any putrefactive change; that the albumen-digesting ferments of the gastric and pancreatic juices were not impaired by calomel, nor the fat-digesting and amylolytic ferments of the pancreas, but that it arrested the changes which followed their completion, preventing entirely, for example, the butyric acid fermentation and putrefactive processes.

Calomel was for a long time used in all chylipoietic or gastro-intestinal maladies with great, even with excessive, confidence; then it was greatly depreciated, and now again it is finding favor with practical men. It has been used in nearly all diseases, and in many with praise and censure about equally distributed. In 1849, while attached to the U. S. Naval Hospital at Norfolk, I saw many cases of Asiatic cholera, all of which were treated with full doses of calomel, and with a remarkably small mortality. Calomel was our prime remedy; all others were but adjuvants. I certainly placed great faith in it; probably too much, for there were other factors of exceeding importance. In a well-ordered naval hospital the patients were all under strict discipline, were never crowded or improperly fed; they were kept rigidly at rest, with all their wants properly attended to, at all hours of the day and night, from the beginning of the attack to complete convalescence. No doubt in crowded or filthy dwellings, or hospitals, or ships, ill-cared for and negligently managed, many cases would have died after taking calomel, or any other, or all the other drugs of the pharmacopoeia.

But I state the fact as it occurred; we had between fifty and sixty cases, all treated with calomel, *inter alia*, and the mortality was so small that we had no reason to distrust the treatment. There were but two deaths.

¹ Amer. Journ. of the Med. Sci., April, 1883.

In quite another search I fell by accident upon an article by Dr. Gayley, in the *Amer. Journ. Med. Sciences* (July, 1850), in which he speaks of having treated thirty-five cases with three deaths only, all under calomel treatment, and he quotes Dr. House, a missionary physician at Bangkok, who treated forty-five cases with only two deaths, to all of which he gave calomel in large doses (3j—3ij), repeated about once an hour, until three or four, or, in one instance, seven doses were taken. The incredulous may readily say that the patients survived the calomel, but were not cured by it. But it happened that large numbers were dying at Bangkok at the same time under other methods of treatment.

Dr. Morehead, in his excellent work on diseases of India, gives the general rate of mortality from cholera as from thirty to thirty-five per cent. in regimental hospitals, fifty to fifty-five per cent. in European general hospitals, and sixty to sixty-five per cent. in civil hospitals in the large towns.

He speaks adversely to calomel treatment, but in his published cases there is no evidence of a lessened mortality when this agent was not in use.

Thus in notes of forty-five cases presented, in twenty-nine fatal, sixteen had no mercury; and in sixteen recoveries, fourteen had mercury, generally calomel, although in some instances it was in the form of blue mass, and in others *mercury and chalk* is noted.

The author's objection to calomel is that it is an irritant to the gastro-enteric mucous membranes, but surely it is less so than many agents freely used, as aromatic spirit of ammonia, turpentine, brandy, wine, etc.

Certainly, calomel or no calomel in epidemic cholera is still open to consideration. It may become before a great while a practical rather than a theoretic question. Probably enough its effective agency is that of a germicide, most useful when given freely in the *first stages*, destroying the special bacilli, which, according to Koch's investigations, exist in the intestinal tract, *but not in the blood*.

In the forms of indigestion in which calomel acts so well, especially when attended by the flatulence of fermentative or putrefactive processes, we may infer its *modus operandi* from the statement previously presented in regard to its control over such developments, while not hindering the physiological action of the gastric and pancreatic juices. Furthermore, it starts a flow of bile, which has its own action, from the ducts and gall-bladder, and thus induces derivation from the liver, even if the drug is not a direct stimulus to the secretory cells of that organ.

Other uses of the chloride of mercury I shall not here dwell upon, but I may repeat what I have published before, that the use of the bichloride of mercury used locally and constitutionally in my practice in diphtheria for years has been attended with the most satisfactory results. Some children, indeed, cannot tolerate even small doses, on account of gastric irritability; in such cases it seems not advisable to push it constitutionally, though it still may be used locally, in spray or gargle. It is hardly necessary to add that the success of the practitioner depends largely upon the hygienic conditions and

surroundings of his patients. A curable case in a comfortable home might readily prove incurable in a crowded tenement-house.

Chlorine in its various other combinations deservedly finds much favor with practitioners in the allied diseases of diphtheria and scarlet fever. This is so much the case that muriatic acid, muriated tincture of iron, chlorate of potassium, and, recently, chlorate of sodium, are in almost universal use.

In my earlier practice, when Watson supplanted Cullen's *First Lines*, Mason Good, and Eberle, I was led to try the *chlorine mixture* in scarlet fever, as commended by Watson, and with excellent effect, but it had no obvious advantages over a dilute solution of hydrochloric acid, which is habitually very serviceable, and, indeed, always appropriate, except when, perchance, it proves irritant to the stomach and intestinal canal.

I do not propose to enumerate what or how many diseases are eminently benefited by the chlorine preparations, but I will make a passing commentary on one or two forms. In the U. S. Dispensatory it is stated that Dr. Christison and Dr. Elliotson "have obtained in consumption a more decided improvement of the symptoms by the use of chlorine inhalations than by any other means."

Independently of so high authority, presumption is in favor of such agency. Let us suppose a pulmonary cavity without protecting epithelium or limiting membrane wherein bacilli may abound and multiply. If they be causative, it is important to destroy their vitality and arrest multiplication. The chlorine vapor, though irrespirable in quantities sufficient to destroy them entirely, may greatly restrain multiplication, having some such power as carbolic acid and alcohol have (Sternberg), and, *quoad hoc*, relieving symptoms and tending to cure.

I shall here introduce but one other chlorine combination, which has remarkable properties. I was called into consultation lately with Dr. Hundley of this city in a case of Bright's disease, marked by albuminuria, hyaline casts, and general cachexia. The doctor had used upon this patient the chloride of gold and sodium in doses of gr. $\frac{1}{10}$ as recommended by Prof. Bartholow, *ter in die*, with such good effect that his patient had left his bed and returned to his ordinary business pursuits. He seemed to be convalescent, but in a few weeks he was housed again with gastric catarrh, hepatic tenderness, and other perturbations, *plus* albuminuria. I suggested a course of treatment which gave the patient some relief, but he requested a resumption of the little pills (the chloride of gold and sodium), which we consented to give, and which again had a very beneficial effect. Cure was not probable, but our patient always defeated his own chances by too much indulgence in strong potations, utterly ignorant or regardless of what Anstie called the *poison-line*, whenever he left the house.

The chlorides have great potentialities in doing harm as well as in doing good (*nil prodest quod non possit ledere idem*). It may be a proof of the utility of calomel when properly used that it was so fearfully abused some years ago.

I saw a man seriously ill a few weeks ago from a

terrible salivation, resulting from the use or abuse of calomel as recommended by a non-professional friend and fellow-sufferer. It is my opinion, however, that my patient never had syphilis, though he always gave most confidence to such advisers as most strongly confirmed his monomania upon the subject.

Of course, the chlorides may be very effective poisons. There is at least one case of a man being fatally poisoned by so useful, and ordinarily so harmless an agent as the chloride of sodium.

While writing this, I observe, in a medical journal, a case of death from chlorate of potassium, where a man took a teaspoonful every two hours until he took in all nearly two ounces. (Dr. Bohm.)

Dr. Johnson's cases, already spoken of as detailed in the *Sanitarian*, are very instructive and interesting. Six persons in one family were dangerously poisoned who had partaken together at one meal of canned tomatoes. Within two hours all were ill. Burning in throat and stomach, retching, tenesmus, gastro-enteritis, and coma, with some, soon followed in order. Epileptiform convulsions; dark, tarry stools, a colliquative sweat, and hemorrhages from the bowels happened in the worst cases. In one case a severe skin eruption, in another partial paralysis of the left arm, and a threatening swelling in the left iliac fossa. All were very ill, but the oldest daughter, who had soaked bread in the liquid part of the food, was most deeply affected. The question was, What was the poisonous agency—spoiled tomatoes, lead, copper, or other matter? Some of the symptoms may come from any of those agents, but not all of them. The tomatoes were gone, but the can was left, and it was noted that the tin was corroded about the open edge. Dr. Johnson took the can to a tinsmith, who at once explained the corrosion. The cap of the can was secured to the head, not by a resin amalgam, but by an amalgam made of the *muriate of zinc*. The poison was, in fact, a muriate of zinc and tin, and the acid around the cap had eaten off the tin and had permeated the contents, and principally the liquid portion. The girl who had eaten this had suffered much more than her brothers, who had eaten the tomatoes from the lower part of the can.

The danger of using the muriate of zinc amalgam, or flux, is so well known in Maryland that its use is forbidden by law. It is to be hoped that the danger will be equally well known and guarded against henceforth wherever canneries are in operation.

The preceding illustrations are intended to indicate, by suggestions rather than in detail, the capabilities of chlorine and its combinations as germicide or disinfectant, as therapeutic agent, and as an overt or insidious poison. Few agents are more valuable in medicine, and not many are more dangerous if improperly used.

BALTIMORE, June 24, 1884

A PRESUMABLE ETHER-DEATH FROM HEART FAILURE.

By JOHN B. ROBERTS, M.D.,
SURGEON TO ST. MARY'S HOSPITAL, PHILADELPHIA.

DAINGEROUS symptoms from ether anæsthesia are usually respiratory; hence it has become too much

the custom to watch the breathing rather than the pulse during the inhalation of ether. In the *Philadelphia Medical Times*, June 4, 1881, I reported, from the practice of a friend, a case of anæsthesia in which danger was first perceived through the circulatory system, and in which death from heart failure occurred. I again report an instance of death from cardiac symptoms arising during etherization. In this clinical history there are more elements of doubt than in the other, for, as post-mortem inquiry was unattainable, death may be attributed to other causes than ether anæsthesia.

Venous thrombosis and embolism, internal hemorrhage from unrecognized visceral injury, and fat embolism have presented themselves to me as possible causes of the fatal issue. I have, therefore, designated the case one of Presumable Ether-death, since it is in my opinion impossible to arrive at the exact cause of the fatal result.

A few days ago a fat German, about fifty years old, the driver of a beer wagon, was admitted to the surgical ward of St. Mary's Hospital with a large and deep laceration of the right thigh. He had slipped while attempting to jump upon his wagon, but the exact method of receiving the injury is unknown to me. When I saw him a couple of hours after admission he had a rather feeble pulse of about 100, but presented no evidence of marked shock. He was perfectly conscious and rational, had a good color, and was losing no blood of importance. The character of the wound makes it probable, however, that quite profuse hemorrhage had taken place immediately following the receipt of injury. On admission the thigh was firmly bound by a handkerchief, which did not, however, stop the arterial pulse below. I found that the wound, which was a simple tear, extended around the inner half of the thigh from front to back, exposing the muscles at the apex of Scarpa's triangle, and laying bare about an inch and a half of the femoral artery and vein. A good deal of loose clot occupied the irregular spaces among the torn structures.

After feeling the pulse, I decided it proper to give an anæsthetic, in order to examine the wound thoroughly and secure any dangerous vessels before suturing. I at the time said to myself that ether would probably strengthen the feeble pulse. Ether was given by Dr. Frank V. Cantwell, one of the resident physicians, who produced anæsthesia with but a moderate amount of excitement and struggling. I removed all clots from the wound, washed it thoroughly with carbolyzed water, and finding no vessels demanding ligature, placed a strand of horse-hair drainage material in it and proceeded to apply sutures. During this time anæsthesia was kept up in a rational manner; that is, when the patient began to snore after the first use of ether the towel was temporarily withdrawn from his face, to be re-applied as he began to show signs of sensation by moving his legs. When three sutures had been adjusted, the etherizer said something about a weak heart. I looked up from the seat of operation, and, as I had frequently done before, glanced at the patient's face, which to my astonishment presented a death-like pallor. Respiration had nearly ceased.

There had been at the time no stertor, and no rattling of mucus in the bronchial tubes. No vomiting had taken place at any time. Immediately the patient's tongue was drawn forwards and held so by means of a tenaculum, while his body was inverted by two assistants standing upon the bed and holding up his legs. The head, hanging over the edge of the bed, was steadied by me sitting upon the floor. Artificial respiration was at once instituted by a third assistant, who elevated and depressed the arms while the patient was kept thus inverted. This position was maintained for nearly an hour, during which time nitrite of amyl and ammonia were administered by inhalation, and sulphate of atropia (gr. one-fiftieth) and tincture of digitalis (ʒij) hypodermically. He rallied sufficiently to become perfectly conscious, struggled against our manipulations, and repeatedly asked for water. The cyanotic appearance of the ears persisted, however, though the lips and face assumed a more natural color. Voluntary respiration became at one time pretty well established, but the feeble heart and fluttering pulse continued. Ten minims of aqua ammoniæ diluted with an equal bulk of water were injected into the arm. Afterwards the condition became so desperate that I injected, at intervals, five hypodermic syringefuls of aqua ammoniæ and water into the femoral vein exposed in the wound. The amount of ammonia thus used was fifty minims diluted with an equal amount of water. Owing to the difficulty of inserting the needle with accuracy because of the position of the patient and the want of light in the wound, some of the fluid did not enter the vein. It was thought that a slight increase in strength of the radial pulse was noticeable after the successive intravenous injections of ammonia.

The interrupted current was applied to the chest walls for a few moments, but was soon discontinued, as respiration was more effectually kept up by raising and depressing the arms, and danger seemed to threaten from circulatory rather than from respiratory failure.

After these various measures had been carried on for about an hour, the patient was placed horizontally upon the bed, but this position was followed by greater depression, and the inverted position was resumed. Progressive failure of circulation and respiration continued, unconsciousness finally supervened again, and death occurred about an hour and a quarter after the first signs of danger. No autopsy was obtainable.

Dr. Cantwell, who etherized, reports that he had poured upon the towel in all about five fluidounces of ether; that early in the administration the rectum was unconsciously emptied of semi-liquid feces, and that before he noticed change in pulse or respiration the small capillaries of the florid face lost their red color.

The fatal termination in this case might be attributed to fat embolism, but the open and gaping wound and the short time after injury make such an explanation improbable. Internal hemorrhage from a large vessel partially divided at the time of receiving the blow or crush which injured the thigh may have occurred, and produced sudden collapse during

the progress of anæsthesia. This does not seem highly probable because there was no violent struggling, but at the time the patient lay in a quiet sleep. Another explanation is more tenable. After the laceration of the numerous veins upon the inner side of the thigh, among others the internal saphenous, the formation of a large thrombus therein probably took place. The moderate struggles of the early stage of anæsthesia and the manipulations by which clots were removed and the wound rendered aseptic could readily displace portions of this thrombus, which might be washed up into the circulatory current as emboli. Such emboli would be most likely to lodge in the lungs and give rise to pulmonary symptoms, such as dyspnoea and cyanosis, which would be expected to show themselves while manipulations in the wound were in progress. The clinical history, however, shows that the initiatory symptoms of danger were cardiac, and arose while the exterior of the wound was being sutured.

The most probable cause of death, therefore, seems to be cardiac failure due to the toxic effects of ether. My former experience in watching such a death makes me more willing to accept this explanation. While the case was under treatment I felt so assured of the symptoms being due to ether that I abstained from the use of alcoholic stimulation, employing atropia, ammonia, amyl, and digitalis for their tonic cardiac action.

CASE OF DEATH FOLLOWING THE INHALATION OF CHLOROFORM.

Reported by P. L. HILSMAN, M.D.,
OF ALBANY, GEORGIA.

MISSOURI W., mulatto, æt. 37, laundress, well developed, weight 160 pounds, and apparently in robust health. On May first, she went by appointment to office of Dr. Osborn, dentist, to have her teeth extracted under the influence of an anæsthetic, having taken a light breakfast early, four hours before, as directed. After making an examination of thorax and satisfying myself that the heart and lungs were normal, I administered chloroform from a cone rudely constructed out of an unstarched towel. Dr. Osborn desired to remove the entire upper set, and also two posterior roots from the lower jaw. The latter being difficult to reach, he requested that she be only partially anæsthetized, in order that she might aid him in her voluntary efforts in the extraction of the roots.

Patient took the chloroform without fear, and when I thought her sufficiently anæsthetized, she was requested to open her mouth, and one of the roots was extracted. She resisted the operation, using her feet so violently as to require an assistant to hold them. She was requested to open her mouth a second time, and Dr. Osborn began to adjust the forceps to the other root, the chloroform being placed as near to her face as I could get it, in order that its effects might be kept up, as we desired to get her fully under its influence before extracting the upper teeth. During the efforts to adjust the instrument to this root, the patient resisting, she suddenly gave a sigh and ceased to breathe. She was at once taken from the chair and placed upon the floor, and artificial respira-

tion used; the tongue was drawn out, and flagellation over the thorax with cold cloths was also used. Respiration gradually returned, though it was irregular and stertorous in character. There being no corresponding impulse in the heart, we used the galvanic battery, but without exciting in the faintest degree its action. Respiration continued for a brief time, and patient died. No post mortem was allowed.

Remarks.—Death appeared to result from paralysis of the heart, induced by the toxic effect of the chloroform. The chloroform was used from a fresh pound bottle put up by W. H. Schiefflin & Co., of New York, and stands the test for purity. I anæsthetized another patient from the same bottle without any unpleasant consequences.

The peculiar feature in the case is that the patient was not brought fully under the influence of the drug before the fatal issue. Not exceeding one ounce was used, and a great part of this still remained saturated by the towel.

A TREATMENT OF SCROFULOUS CONJUNCTIVITIS, ASSOCIATED WITH NASAL CATARRH.

BY LOUIS KOLIPINSKI, M D.

OUT-DOOR AND LATE RESIDENT PHYSICIAN AT THE CHILDREN'S HOSPITAL, WASHINGTON, D. C.

THE treatment of conjunctivitis is fully described in works on ophthalmology. Graefe and Saemisch introduce the subject well in saying, in the article on "Conjunctivitis Catarrhalis Simplex:" "The therapeusis of catarrhal conjunctivitis has to investigate the cause of the inflammation, and, if possible, to remove it" (*Handbuch der Gesamt. Augenheilkunde*).

The writer of the article "Cornée" in the *Dict. Encyclop. des Scien. Médicales*, tome xx. p. 498, speaking of the pathology of the subject, carefully prepares to meet this persistent and recurring enemy: "Our first precaution to take against phlyctenular keratitis is to destroy its complications. This is a malady which, according to the happy expression of Bowman, applied, it is true, to another subject, it is necessary to attack, like a fortress, by successive approaches. We must cure the nasal and labial erosions. . . . We must cleanse the nostrils. . . . We must finally examine the lachrymal passages. There is a last thing which we must not pass over or neglect. It is to assure ourselves that there is no point of permanent irritation at the extremities of the fifth pair."

The quotations show that authorities recognize the fact that a coryza should be diminished or cured, although no statements have been met with regarding the results obtainable when no other local or general remedies are applied.

It is not the intention in this short article to discuss the mode of origin or the causes of the disease. It is simply offered, if for no better reason, that the treatment may be used to permit a careful and patient examination of the eyes of a struggling, unmanageable child.

During the past winter and early spring, there were treated in the hospital and dispensary services of Dr.

W. V. Marmion a number of cases of conjunctival inflammation, associated with phlyctenular ulcers of the cornea. They possessed in the usual marked manner the symptoms: photophobia, blepharospasm, and lachrymation. A nasal catarrh with free mucous discharge was coexistent. In some cases both eyes, in others but one was affected. In the latter instances, the coryza was most marked in the nostril on the same side as the affected organ. The histories stated that the disease had remained as a sequel of measles, whilst in others in whom the acute infectious disease had occurred some years before, or not at all, the accounts indicated an antecedent nasal catarrh. The duration of the eye-affection had been varying—from one to two weeks, or as many or more months. A few of the children were scrofulous; the rest gave but little evidence of constitutional vice or debility.

The treatment in the nineteen cases consisted in having the patients snuff up the nostrils, three or four times daily, a solution of fifteen grains of potassium chlorate in one fluidounce of water. In those too young to do this thoroughly, and in the obstinate, the same solution was dropped from a phial or pipette. The use of the remedy was found to be agreeable to the patients. Local treatment of the inflamed surfaces was omitted. The distressing symptoms, which necessitated the wearing of a shade or bandage, disappeared in from one to three days. At the next visit, the patients invariably presented themselves with the eyes tolerant of light, and the deeply injected mucous membrane of a much more normal hue.

The nasal discharge was lessened; incrustated mucus removed, and the excoriated upper lip in an improved condition.

Whilst this favorable state did not in all instances terminate in as rapid a cure as the speedy amelioration might have caused one to hope, yet in some instances this really happened.

The two following histories may serve as a type:

CASE I.—April 21, 1884. H. P., male, black, aged ten years. Has been delicate since birth. In Nov. 1883, had measles. The eyes, which had been previously affected, grew worse thereafter. He is compelled to wear bandage and shade, and cannot tolerate daylight. *Examination:* nutrition fair; discharge from both nostrils; upper lip swollen and excoriated; blepharitis, conjunctivæ inflamed; phlyctenulæ on both corneæ; in the right eye an ulcer near the centre. After two days the boy was at play. His eyes have given him no discomfort since. A slight opacity marks the spot of the corneal ulcer. He is treated for the marginal inflammation with an ointment of the yellow oxide of mercury.

CASE II.—April 28, 1884. W. T., male, black, aged eight years. Has had sore eyes for about two months, with a discharge from the nose. Has always been healthy. Has not had chickenpox, measles, or scarlet fever. Mother healthy. Father whilst young was scrofulous, now has consumption. The nasal discharge is worse from the right side. Skin about anterior nares excoriated, inflamed, covered with bloody crusts, and bathed in a fluid discharge. The conjunctivitis more intense in right eye. Eyelids thickened, epithelium rubbed off. An ulcer on

right cornea near limbus. Was treated for last six days by a physician with some collyrium. Condition not improved thereby. Has great intolerance of light, and, although a bandage is worn, still holds his head in his hands, and constantly wipes away the flow of tears. On the third day of treatment is completely relieved. No discomfort or suffering. A week after is discharged cured, the nostrils in the meanwhile having been cleansed once or twice daily.

Whilst these may represent the more successful cases as regards a cure, yet the symptoms which are so characteristic of the affection were invariably relieved. The following is an example:

CASE III.—March 28, 1884. H. C., female, black, aged nine years. Eye sore for about one month. Small ulcer on left cornea. Atropia instillations.

April 4.—Much improved. An ointment of the yellow oxide of mercury substituted.

11th.—No photophobia; conjunctivæ acutely hyperæmic; pupil moderately dilated.

25th.—Eyelids slightly swollen; conjunctivæ very red; lachrymation and great photophobia; discharge from left nostril. Has not attended service regularly. The solution of potassium chlorate directed to be snuffed up the nostril. No other medication.

27th.—Can bear light without the shade or bandage; conjunctiva much lighter in color; no pain, no discharge of tears; condition of comfort has remained; ulcer shows no tendency to heal. Applied topical remedies again. The lesion begins to get well, no nasal discharge.

With what pleasure could one not view the timid, faltering, light-fearing patients restored in so short a time to assured and contented beings.

HOSPITAL NOTES.

CINCINNATI HOSPITAL.

Service of N. P. DANDRIDGE, M.D.

TWO CASES OF EXTERNAL PERINEAL URETHROTOMY WITHOUT A GUIDE.

(Reported by W. H. WILDER, M.D.)

CASE I.—Mike S., laborer, aged fifty-three, was admitted to the Hospital on the evening of December 30th, and gave the following history: About five weeks before the date of admission, while engaged in work about a building, he fell astride a joist, and soon after the accident experienced severe pain in the perineum; and noticed extensive extravasation of blood into the tissues of the scrotum, thighs, and abdomen. He stated that no hemorrhage occurred from the urethra at the time of the accident or at any subsequent period; and said that micturition had been accomplished readily up to a time about thirty-six hours before his admission, but that the stream had steadily decreased in size. When admitted, he said that the bladder had not been emptied for thirty-six hours, and he was suffering from intense pain from distention of that organ, and the dribbling consequent upon it, the "incontinence of retention." The dulness of the bladder extended up to the umbilicus. In the median line of the perineum, immediately below the scrotum, there existed a swelling

about the size of a walnut, which was tense, red, and fluctuating, and suggested the thought of an abscess, which was dispelled by the fact that the swelling increased in size when the patient was in the erect, and diminished when he was in the prone position. This fact, taken with the statement of the patient that the tumor became larger during the attempts at micturition, seemed to indicate strongly that it connected with the bladder through a lacerated urethra. After several ineffectual attempts to introduce even the smallest catheter or the smallest filiform bougie, suprapubic aspiration of the bladder was resorted to and thirty-eight fluidounces of urine were withdrawn, affording the patient instant relief, to which he testified by most profuse thanks.

The urine dribbled all night, probably on account of the temporary atony of the bladder wall and sphincter muscle occasioned by the prolonged distention. On the morning of December 31st an attempt to pass a filiform bougie into the bladder was made, but this failing, external perineal urethrotomy without a guide was performed, the incision being made through the swelling above alluded to, which proved to be full of urine. The bladder was readily entered at this point, and the stricture was then divided in its whole extent. An enema was ordered and the wound dusted with iodoform. In the evening of the same day the pulse was 84, temperature 98½°, and the patient complained of no pain.

In the afternoon of January 1st, he had a severe chill followed by a rise of temperature to 104°. Sponge bath and gr. xx of quinine were ordered.

January 2.—Much better; pulse 100; temperature 99¼°. Urine was passed entirely through the cut. P.M. pulse 84; temperature 98.4°. From this time he improved, and the incision in the perineum gradually healed, the urine being passed partly through the cut and partly through the urethra. From January 10th a No. 15 English sound was passed daily until January 17th, when he was discharged. Urine was passed naturally, and the wound in the perineum had closed.

CASE II.—J. S., miller, aged twenty-nine, was admitted to the Hospital December 7, 1883, with a traumatic stricture of the urethra which was the result of a laceration of that canal caused by a fall astride a plank nearly one year ago. He stated that this accident was followed by bleeding from the urethra, swelling of the scrotum, perineum, and adjoining parts with ecchymosis; that his urine was drawn with a catheter for two weeks subsequent to the accident. His recovery, which was rapid, was followed by symptoms of stricture which continued to increase up to the time of his admission, when urine could be voided only by drops, micturition was accompanied by severe pain, and the smallest sound could not be introduced into the bladder.

On December 8th, Dr. Dandridge, after a careful but unsuccessful attempt to introduce a filiform bougie, performed external perineal urethrotomy without a guide. A Syme's grooved staff having been introduced down to the point of the stricture, was cut down upon and the contracted portion of the urethra laid open in its whole extent. This dissection was facilitated by introducing thread through the lips of the incision into the urethra, which enabled the assistants to keep the wound open,

giving a clearer view of the parts involved in the operation. After dividing the stricture, which existed in the anterior part of the membranous portion of the urethra, a large sound was easily introduced into the bladder. The wound was left open. Patient had no pain after the operation, bowels were moved by enemata, and the urine was passed partly through the cut in the perineum and partly through the natural channel. Four days after the operation a No. 24 French sound was easily passed into the bladder, but a No. 27 failed to enter. From this time the wound healed kindly and rapidly; the patient's general condition remained good and urine was more frequently passed through the urethra. A No. 27 (French) sound was passed daily into the bladder until the date of his departure.

When discharged, December 29th, a very small opening, nearly healed, existed in the perineum, but the urine passed entirely through the penis. He received the injunction to have a large-sized sound passed into the bladder at regular intervals of one week for the next year, and after that once a month.

Remarks.—Both of these cases illustrate that form of stricture termed traumatic, impermeable stricture, and the operation necessary to give relief—external perineal urethrotomy without a guide. In neither case could the smallest filiform bougie enter the bladder, otherwise the operation would have been simplified.

The seat of the stricture in both cases was in the membranous portion of the urethra, and it is very probable, from the description of the accidents—a fall astride a joist and astride a plank—that the injuries were caused by the membranous urethra being driven violently against the sharp edge of the subpubic ligament and thereby sustaining a complete rupture of the mucous membrane together with the fibrous and muscular coats, as in Case II., or a rupture which left the mucous membrane intact, as was probably the condition in Case I.

This seems to be indicated by the distinct and repeated declaration of Case I. (who maintained it positively) that no blood had escaped from his penis; but there were considerable swelling and ecchymosis of the surrounding parts. In Case II. hemorrhage from the urethra occurred immediately after the accident. The swelling in the perineum alluded to in the first history, which occurred a few days before his admission, and not soon after the accident, was probably the result of rupture of the mucous membrane during the process of repair, and the extravasation of urine into the tissue between the two layers of the deep perineal fascia. The urine could not pass forwards on account of the attachment of this fascia to the rami and symphysis of the pubes. The presentation of this prominent mass, and the knowledge that it contained urine, rendered the dissection along the constricted track of the urethra much easier than it was in Case II. The dissection is necessarily difficult, but too much cannot be said in praise of the use of threads passed through each lip of the incision in the urethra, by which the wound is kept open and clearly in view without the intervention of retractors. The threads should be double and looped, so that the assistants at each side can hold the wound open. Since the method of treating the wound after external perineal urethrotomy, by leaving it open, has superseded

the old plan of leaving a catheter in the bladder for a week or two to become incrustated with urinary salts and retard the granulation of the wound, better results have been obtained from this operation, and fewer perineal fistulae are seen in consequence of it.

MEDICAL PROGRESS.

CHOLERA NOSTRAS.—DRS. J. PRIOR and D. FINKLER have just published, in the *Deutsche medicinische Wochenschrift*, of September 4, 1884, the results of their researches as to the nature of cholera nostras. These researches were made in the Medical Laboratory at Bonn, the experimenters having already made preparation for this study before the recent report of Dr. Koch on Asiatic cholera.

The first case which occurred at Bonn, on July 14, was that of a man who was suddenly taken ill, without previous symptoms of indigestion, with headache, vomiting, and diarrhoea. For two nights there had been cramps in the legs; the stools were clear and thin. The tongue was gray, choleraic voice, cold hands and feet. Skin clammy; pulse was regular, weak, and 112. The case was diagnosed as cholera nostras, and treated with Russian cholera drops and mustard plasters. On the following day he was apparently well. Within a short time after the occurrence of this case, five others occurred in one house, and three in another house not far off. Then the cases seemed to spread somewhat, and altogether twenty-nine cases occurred. One would naturally think that the first two houses had been centres of infection from which the disease spread. All of these cases recovered, and the patients regained their normal strength very quickly. In many cases the diarrhoea was almost immediately cut short by the cholera drops, so that scarcely any opportunity was offered for examining the stools. The stools were at first fecal, but soon became thin, watery, and clear, and contained mucous flocculi. Entirely colorless stools were not observed, however. It seemed very probable that the infectious material could be found in these rice-water discharges, and they were, accordingly, examined.

The first examinations were made in the usual manner with cover-glass preparations of dried particles. Nothing characteristic was found in these specimens. The early discharges were then examined, with the result that bacilli were found, which had a most marked resemblance to Koch's comma-bacilli, in size, shape, and general appearance. It was also especially noteworthy that these bacilli were often found in nests. Occasional gaps in the preparation which let in light, as crystals in a gland, were found to be filled with these bacilli, and here their contour could be plainly distinguished. No cocci, or only very few, were found in the immediate vicinity of these places; in fact, in the best preparations, in which the comma-bacilli were very abundant, other bacilli were very scarce.

As Koch has stated that the cholera bacilli are soon killed by drying, and that they do not color well when dead, Finkler and Prior experimented with the view of coloring the morphological elements of the discharges before they had been dried at a high temperature. A single drop was placed on a cover-glass, and subjected

to a low temperature in a current of air; and another series of preparations were made by throwing the discharge directly into aniline, and after allowing it to remain therein for some time, specimens were taken out and dried on a cover-glass. The bacilli were not colored well by the latter method; but on examining the preparations a great number of large spirilli were found. They were at least five times as long as the bacilli, tolerably thick, the ends generally thinner than the middle part. These have never before been found in any intestinal discharges, at least by the writers. In some preparations both the comma-bacilli and the spirilli were found; and forms were seen which occupied a middle ground between the two, which seemed to be related to both. They especially emphasize the fact that they have never encountered such forms in any previous examinations. Normal stools do not contain them; nor do the discharges from typhus, tuberculosis of the intestines, diarrhoea of children, etc., contain them.

In view of these facts the following propositions are set forth:

1. They have found, in the discharges from persons who had cholera, which, in view of the manner of the outbreak and the absence of spreading, they must believe to have been cholera nostras, comma-like bacilli which very closely resemble the bacilli of Asiatic cholera, found by Koch.

2. In the stools from the same cases large numbers of spirilli were found.

3. These were the only objects of a specific nature which were found.

Culture-experiments were undertaken with small particles from the discharges placed some on moist linen, and others on pieces of potato. These were examined after forty-eight hours, and it was found that the bacilli had entirely disappeared, and only some small micrococci were observed. This was the result in about fifty cultures. It was evident, therefore, that some other method than that employed by Koch for ascertaining the nature of the bacilli must be used. The discharges were fed to dogs, but with entirely negative result.

As to the question, whether these were cases of cholera nostras or Asiatica, it must seem that the evidence is in favor of the former. The outbreak was not strictly an epidemic, and none of the cases died. Koch has reported that his examination of the discharges from a very rapidly fatal case of cholera nostras showed no comma-bacilli. But it must be remembered that Finkler and Prior also failed to find them in the fluid discharges from their cases; but when the early discharges were examined the bacilli were found. Hence, it must be concluded that they were carried out of the intestines by the first discharges, and the results of Koch's examination of the specimens in question must be thrown entirely out of the discussion.

Prior and Finkler conclude, therefore, that the diagnosis between cholera nostras and cholera Asiatica cannot be settled entirely by the microscope. As to the results of the culture-experiments, it is possible that, while the bacilli found in the cases of cholera nostras were similar to those described by Koch as occurring in Asiatic cholera, they may differ in their biological properties; and it is highly important that this should be definitely determined in one way or the other. Of

course, the entirely negative result of feeding the discharges to animals has no weight whatever; as this is also true of the discharges from Asiatic cholera.

LUXATION UPWARDS AND FORWARDS OF THE STERNAL END OF THE CLAVICLE.—Luxations of the sternal end of the clavicle are rare; and dislocation upwards and forwards is so rare that Polaillon had never seen one up to 1875.

DR. PAUL FABRE reports the case of a young man, eighteen years of age, who received an injury while attempting to uncouple cars in a mine, with his right hand, the cars being in motion. He was struck on the right scapula and shoulder, thrown around, and his left shoulder was struck against the gallery-wall. Fabre found that the sternal end of the right clavicle was dislocated upwards and forwards. The luxation was reduced by double counter-extension; one assistant drew the right arm backwards, another drew the left backwards and outwards, and while a third held the patient steady, Fabre passed his left arm around the patient's neck, and with his right thumb pressed the dislocated end downwards and a little backwards. By this means the luxation was quickly and easily reduced. In order to retain the end of the bone in place, a pad was placed in the right axilla, and a dressing, similar to the Desault bandage for fractured clavicle, was made of pads and placed in front of the sternum. A cushion was also placed between the shoulders, and the right arm put in a sling. The patient was well in about two weeks.

The mode of production of this dislocation differs from that of luxation forwards. According to Follin and Duplay, the determining causes are: a fall on the shoulder, and all kinds of violence, which forcibly carry the shoulders backwards and consequently throw the sternal end of the clavicle forwards, and distend and rupture the anterior sterno-clavicular ligaments. The method of reduction employed by Fabre has not been used before.—*Gazette Méd. de Paris*, August 9, 1884.

ECTOPIA VESICÆ.—HOFFMANN has a paper on this subject in the *Mittheilungen aus der Chirurgischen Klinik in Greifswald*, 1884. If the defect of the anterior wall of the bladder is covered by flaps from the skin of the abdomen, the anterior wall thus made is covered by granular tissue or epidermis at most; which, in Hoffmann's opinion, is very incomplete.

Vogt attempted in one case to form the whole bladder from the mucous membrane, by loosening the excess of mucous membrane from the anterior wall and making a sac. Over that he brought a flap from the abdominal wall. In order to keep the urine away from the fresh wall a Nélaton's No. 5 catheter was placed permanently in each ureter, a hole having been burned in the end of the catheter with a hot needle. The skin-flap was, to a great extent, gangrenous. Nothing is said as to the state of the wound. The patient, a girl seven years of age, died in ten days of double purulent inflammation of the ureters and extensive chronic pyelitis.

Although the permanent drainage of the ureters caused death in this case, Vogt wished to use the method in another. The patient was withdrawn from treatment, however, after the urethral canal (Eichelkanale) was finished.—*Centralbl. für Chirurgie*, August 2, 1884.

THE MEDICAL NEWS.

A WEEKLY JOURNAL
OF MEDICAL SCIENCE.

COMMUNICATIONS are invited from all parts of the world. Original articles contributed exclusively to THE MEDICAL NEWS will be liberally paid for upon publication. When necessary to elucidate the text, illustrations will be furnished without cost to the author. Editor's Address, No. 1004 Walnut St., Philadelphia.

SUBSCRIPTION PRICE, INCLUDING POSTAGE,

PER ANNUM, IN ADVANCE, \$5.00.
SINGLE COPIES, 10 CENTS.

Subscriptions may begin at any date. The safest mode of remittance is by bank check or postal money order, drawn to the order of the undersigned. When neither is accessible, remittances may be made at the risk of the publishers, by forwarding in REGISTERED letters.

Address, HENRY C. LEA'S SON & CO.,
Nos. 706 & 708 Sansom Street,
PHILADELPHIA, PA.

SATURDAY, SEPTEMBER 27, 1884.

COAGULATION-NECROSIS.

COAGULATION-NECROSIS is a term suggested by Cohnheim for a condition as to the precise nature of which, it would seem, pathologists are not entirely in accord. In his recent work, *The Elements of Pathology*, PROF. RINDFLEISCH says, "At the present time diphtheritic inflammation is defined to be an inflammation in which the lodgement of cleft fungi has produced, over a greater or less extent of surface, a condition of *coagulation-necrosis*." He goes on to say that this form of necrosis is distinguished from the simple death of a part, in that the transition from life to death in the cells and tissues is accompanied by the coagulation of an albuminous fluid, and that the process bears so striking a resemblance to the coagulation of fibrin, that one is tempted to consider the two as identical. But the fact that the coagulation takes place by preference in the interior of cells and other tissue constituents, produces special microscopic and macroscopic effects.

By the microscope a peculiar homogeneousness of the protoplasm of cells, and a total disappearance of the nucleus are noted. Thus they lose their sharp contour and become flaky masses, which adhere and form larger, irregular, membranous surfaces. Very striking is the peculiar wax-like brilliancy evidently due to their impregnation with a highly refracting solid albuminous substance.

To the naked eye, coagulation-necrosis appears as an opaque desiccation of the dead part. As all the normal tissues, including bone, are partially transparent, and as the necrosis is usually confined to a limited area, we find sharply defined patches, dis-

tinctly separated from the surrounding tissues. These somewhat resemble escharotic crusts or scabs.

Like the eschar, too, the necrosed part produces an irritation, which invariably results in a reactive inflammation with corpuscular exudate. The strictly diphtheritic inflammation is violent, terminates in suppuration, and leads to the throwing off of the coagulated eschar and to the formation of deep-seated ulcers. This ulcer can, by repeated recurrence of the coagulation at the bottom and sides, enlarge and deepen, and even assume a gangrenous or phagedenic character before it begins to heal and cicatrize.

At the recent session of the International Medical Congress at Copenhagen, PROF. WEIGERT, of Leipzig, read a paper on this subject, before the Section of Pathology and Pathological Anatomy, which was noticed in THE MEDICAL NEWS, September 13th, p. 299. He, too, defined coagulation-necrosis as a process by which tissues are transformed into a substance resembling coagulated albumen, comparing it to that forming fibrin and cheese. He says, however, that the change has been erroneously confounded with the coagulation of fibrin, and the cheesy change with desiccation and inspissation of dead tissues. The parts coagulated must be first dead, and also have the appearance of coagulation from a macroscopic aspect. Microscopically, it is characterized by the loss of cell-nuclei, although this may occur also in putrefaction.

As stated, the coagulation must succeed death as its result, not precede it as its cause; the organs must contain substances capable of spontaneous coagulation as do all the protoplasms. The cause may be ischaemia, trauma, chemical agents, or micro-organisms. The victims may be whole organisms, or new formations, or cells. Instances of the first are found in the products of extrauterine foetation; of the second in infarcts, diphtheritic membrane, cheesy tubercle, medullary infiltration of Peyer's glands in typhoid fever, and tumors; and of the third in renal epithelium and leucocytes. The dead and coagulated tissue must be intimately connected with plasmatic fluids, hence must be in the interior of organs, or it must be vascular. Putrefaction and purulent poisons are opposed to coagulation. The fate of the coagulated part is softening, calcification, and the formation of hyaline substance as in waxy degeneration and coagulation of renal epithelium. The hyaline substance, he says, is not a special product.

In the discussion which followed the reading of the paper, Prof. Virchow, who considered Weigert's views somewhat obscure, objected to the name, which, he thought, might suggest that death was brought about by the coagulation. He preferred the term *inspissation*. It will be observed that Rindfleisch, too, speaks of the process as a desiccation, while

Weigert says it has been confounded with desiccation and inspissation of dead tissues. The first act in coagulation, according to Virchow, is the loss of liquid, and is independent of plasmatic fluids. Weigert holds that plasmatic fluids are essential. According to Virchow, owing to a loss of vitality, the water in the tissues is not held, the retention of liquid being an act of vitality. According to Rindfleisch, too, the process consists in the coagulation of an albuminous fluid. Virchow does not believe that the change of tissues to hyaline substance necessitates the absorption of liquid. In the dead foetus the first occurrence is the absorption of the amniotic fluid. There is no coagulation, but an inspissation; no fibrin is formed. Intercellular spaces disappear and the external parts may be subjected to fatty metamorphosis, but the internal parts do not change, as in a placenta he had seen which had been forty years in the cavity of the uterus. A parallel might be found in muscular hemorrhage, where the blood is finally converted into a cheesy mass formed of compactly placed blood-corpuscles, where we surely have an inspissation and not a coagulation. Similar changes take place in cancer cells which shrink, and become dense and homogeneous.

Prof. Weigert admitted a loss of liquid, but claimed that it was primary and not secondary. Cheesy lymphatic glands are not diminished in size. The cells of tumors which have undergone cheesy degeneration, and the cells of cheesy pneumonia are not smaller in size. The loss of liquid is a subsequent event.

It is evident from the above that, according to Virchow's view, the process dare hardly be spoken of as a coagulation, but as a simple inspissation of new-formed tissue. If this is the case, there seems scarcely sufficient reason for the coining of the word. If, on the other hand, it is due to the infiltration of a cell or tissue with an albuminous fluid which coagulates, and that this immediately succeeds death, the word may be a useful one. Yet if this is the nature of the process, we do not think that the desiccated products of extrauterine foetation and the exuberantly produced dead cells of a tumor, or the desiccated aggregate of cells in a hemorrhagic infarct, belong to it. If the term were simply applied to the necrosed and fibrin-infiltrated aggregate of cells which constitute the membrane of diphtheria, there would be good reason for its introduction, for we have no distinctive word for this process, while there are others for almost all the conditions which have been included under it.

A very simple modification of the term, which would obviate the objection of Prof. Virchow that it may suggest that the coagulation is the cause of the necrosis, would be to make it necrosis-coagulation instead of coagulation-necrosis.

CESOPHAGOTOMY FOR FOREIGN BODIES.

DURING the past ten months more examples of cesophagotomy for the extraction of foreign substances have been recorded than were published during the previous five years. We infer, therefore, that surgeons are now fully alive to the fact that the knife should be resorted to immediately upon the failure of efforts at removal with the forceps and other appliances. In such cases tampering is most dangerous, the statistics of Aldemann indicating a mortality of nearly 35 per cent. when the case is allowed to pursue an unaided course, and we trust that the future will show none of the curious examples that have hitherto been recorded, of the coughing up of coins and other extraneous substances after a sojourn of several months.

The most noteworthy contribution of the present year to the operation is from the pen of Dr. LEROY McLEAN, of Troy, New York, who reports in *The Medical Record*, for September 13, not less than five personal cases, of which four recovered, and among them one in which a tooth-plate had been impacted in the tube, at a point eleven inches and a half from the incisor teeth, for upwards of eleven months. In the fatal case, that of a man thirty-six years of age, the plate with two attached teeth, which had been swallowed twelve years and two months previously, was encysted eleven inches from the incisor teeth. It was removed with the forceps; but wheezy and labored respiration, and emphysema of the wound rapidly supervened, and death ensued from exhaustion and dyspnoea in forty-eight hours. The emphysema was due to rupture of the air-vesicles during a violent fit of coughing at the time of the administration of the anæsthetic.

In none of his cases did Dr. McLean close the wound, and he believes that liquid food should be administered by the mouth within a few hours after the operation, thereby doing away with nutritive enemata and feeding through cesophageal tubes. While the latter of these expedients should in our opinion not be resorted to, it is a question whether the partial escape of food by the wound is not equally undesirable, and whether the closure of the opening in the cesophagus, as well as the external wound, by sutures, as first advised by the elder Gross, along with nutritive enemata for the first few days is not a better procedure. In a successful case recorded in the *Transactions of the Medical Society of the State of West Virginia* for 1884, Dr. HOWELL of Clarksburg, employed sutures and rectal injections; and iced water drank on the third day, and milk on the fourth day, indicated primary union of the cesophageal incision, there being no leakage. Were we ourselves called upon to do the operation, we would certainly employ sutures, with the hope of avoiding

the risks of suppuration and the establishment of sinuses in the deep structures of the neck.

The most recent statistics of oesophagotomy for the removal of foreign bodies are those of Mr. Durham, in his article on "Injuries of the Neck," contributed to *A System of Surgery*, edited by Mr. Holmes and Mr. Hulke. Of the 45 cases, 35 recovered, and 9, or 20 per cent., perished, and in two the foreign substance was not found. We have ourselves a record of 71 cases, of which 53 recovered, and 18, or 25.36 per cent., died. In six the foreign body was not found, and in one, that of Sonnenberg, it slipped into the stomach and was expelled per anum. In two instances the fatal issue cannot be attributed to the procedure, as in one, a case of Billroth's, death was due to pulmonary tuberculosis, which existed before the operation was done, and in the second, in the hands of Billé, oesophagotomy having failed in its purpose, gastrotomy was performed, and the man died of peritonitis. Omitting these cases, the mortality of the procedure may be placed at 22.55 per cent., a rate which will surely be very materially reduced if, in the future, the operation be resorted to as early as it should be.

WATER-WASTE AND THE PROPOSED REMEDY.

THERE is no doubt that a large quantity of the water pumped into the public reservoirs is allowed to run to waste, either through gross carelessness or by design, without securing any particular benefit to the consumer by its lavish use. Recent careful investigations made by the water departments of some of our large cities have developed the fact that this waste is excessive, in some instances exceeding thirty-five per cent. of the total quantity distributed. This necessarily increases the water-rates over and above the figure which should suffice for producing a revenue ample enough for all legitimate purposes, and its remedy should very properly engage the attention of a wise administration. The correction of this fault requires the most careful and considerate action on the part of the authorities.

The liberal use of water is so essential to health that it would be better, from a hygienic point of view, to permit of its waste than to impose restrictions that might have a tendency to foster a habit of a too economical use of this most important commodity. If the people are willing to pay for the luxurious use of water, by all means let them have it. At the present time, the amount collected in Philadelphia from water-rents is several hundred thousand dollars in excess of the sum necessary for defraying all the expenses of the department, this excess being diverted toward paying the general expenses of the municipality. And yet there is no general complaint of excessive taxation in this particular, thus showing

that the people are willing to pay freely for what they have the liberty of enjoying freely.

The metre system has been suggested as the proper remedy for the waste of water. By this system the amount of water used is automatically registered, and the consumer is charged at a specified price per 1000 gallons for all water used, and no more. The British Parliament has declined to take so retrograde a step as to permit the water companies to sell water by meter. As above intimated, the adoption of this plan does not deserve encouragement, for the reason that it would to a certainty cause such a restricted use of water as would be disadvantageous to the health and comfort of the people. It may be said of this plan that it is founded upon strict business principles, but it can be just as truly affirmed that it would be unpopular, and attended with disadvantageous results. If there be a need of departing from an old-time custom, let some other method first be tried. All that is desired to be accomplished might be brought about by inspections with the aid of the waterphone, by warnings, and by imposing a fine in cases of reckless waste of water. But even this plan has its objectionable features, as it would be difficult to discriminate between a necessary and a useless consumption of water, and, moreover, the invasion of the privacy of the household necessary to ascertain the fact would be resented by the people.

Bearing in mind the fact that the free use of water is now more than ever indispensable to the health and comfort of the people, it is difficult to see how any restrictions upon its consumption, even for the laudable purpose of preventing waste, can be at all advantageous except in the relatively insignificant particular of a slight reduction in the water-rates.

THE RADICAL CURE OF HERNIA.

IN a paper read at the recent meeting of the British Medical Association, and published in the *British Medical Journal*, for September 6, Mr. BALL, of Dublin, expresses his conviction that torsion of the sac is the best means of bringing about a complete and permanent cure of hernia. The sac, previously isolated from the cord, is grasped with a clamp-forceps high up, and given five or six half-turns, or as many turns as may be necessary to impart the sensation of a decided resistance, which denotes that further twisting would be attended with risk of rupture. The neck of the sac is next firmly tied with an antiseptic ligature; a salmon-gut suture is passed through the pillars of the ring, and also through the sac, to prevent untwisting, and the operation is completed in the usual manner. In effecting separation of the sac from the cord, the three coverings common to both should be thoroughly divided before attempting the isolation of the

former, and nothing should be twisted except the thickened peritoneum and subperitoneal tissue. It need scarcely be said that torsion of the sac can be more easily practised in inguinal hernia of the female and in femoral and other forms of hernia, in which the cord is not present to complicate the dissection.

Mr. Ball calls attention to the fact that in the modern operation for the radical cure of rupture, namely, ligature of the neck of the sac, with excision of the fundus and suturing the margins of the ring, the proportion of failures to the cures is somewhat large. Thus, according to Dr. Guénod, of 34 cases from the surgical clinic at the hospital at Bâle, there was relapse in 12; and in *THE MEDICAL NEWS*, for February 9, 1884, we showed, from the results obtained by the Liverpool surgeons, and from the statistics of Leisrink, that the operation fails in about 1 case in every 5. Hence, any modification of the procedure which will lessen relapses will be gladly welcomed by surgeons, and it is to be hoped that those having large opportunities in this direction will give the new method a fair trial. It should certainly secure a more complete closure of that portion of the sac situated in the inguinal canal than is afforded by the ordinary procedure, and it should also, by tightening and throwing into ridges the peritoneum, overcome the abnormal laxity and fulness of that membrane in the vicinity of the ring.

"OTHER LANDS—OTHER CUSTOMS."

ANY constant reader of the leading English medical weeklies must have been often struck with the difference between the importance attached to various matters in that ancient kingdom and that to which they appear entitled here. Not long since *The Lancet* invited the attention of its readers to the melancholy occurrence of an inflammation of the prepatellar bursa in the person of a clergyman; and a correspondent solemnly urged upon the class to which he belonged the duty of guarding against such consequences of their devotion by having a cushion with them on their visits to the poor, or at least of kneeling upon a folded coat. No place, they were wisely informed, was so beggarly but that in some way the perils of housemaid's knee might there be avoided. An excellent piece of advice, and no doubt much needed where knees are so tender, but one conceived in a spirit which fails to appreciate the solace a clergyman so afflicted would have in the proud consciousness of bearing ever with him a sign which, as well as any other, deserves to be characterized as "good for a clergyman, though bad for a horse."

Again, the *British Medical Journal* has lately called the world's attention to a singular, but more serious state of affairs. Under the caption of "An Ethical Point in Obstetrical Practice," it discusses what it calls "a distressing fatality." It seems that

the wife of a baker had engaged a surgeon to attend her in an approaching confinement. When she fell in labor he came to her, but refused to give her his services because her husband would not leave the room. The surgeon left the house, was sent for again, refused to return, and the woman died before another medical man could be secured.

The temperateness of the comments of our contemporary on this truly "distressing fatality" might be supposed to be attributable to the fact that such occurrences are limited to the tolerably rare occasions when a baker-husband—whose personality we presume is peculiarly dangerous to the parturient female—insists on being with his wife at this critical juncture. But the language of a correspondent, quoted with seeming approval, indicates that the British medical man is impressed with the idea that the presence in the lying-in chamber of any husband whatever is dangerous to the patient and derogatory to the dignity of the physician. However, in view of the consequences to the recalcitrant baker and his—who knows but equally recalcitrant—wife, and perhaps children, the correspondent and the journal seem to feel some pity for these misguided wretches, and to think that circumstances might justify less inflexibility than was shown in this case in maintaining that dear right of the British medical man: to send a husband away from a wife writhing in those perils which call forth the prayers of the highest and lowest in the land in every morning service of the Church of England.

But, as we reflect, a grim question rises in our mind, and will not be silenced: Where would the British medical man be now if the British baker had been an American "cow-boy?"

SPECIAL ARTICLE.

INTERNATIONAL ELECTRICAL EXHIBITION, PHILADELPHIA.

[SECOND NOTICE.]

STATIC electricity is certainly coming more into fashion among sound electro-therapeutists, and the effect of this new departure is to be seen at the Exhibition. Queen & Co., among other displays, have some very fine Toepler-Holtz machines, both imported and manufactured by themselves. One very large wheel, 90 cc. in diameter, is capable of giving a spark 15 inches long. This size is used chiefly for scientific demonstration; the medical machine not being more than 25 cc. in diameter and giving a spark of from 4 to 5 inches. There are also shown machines with four revolving plates, giving a large quantity of electricity. This firm have modified the binding posts, which in the original were connected on one side with the condenser, but which are now fastened on both sides near the knobs.

In the display of this firm we have some very good instruments of precision. There is the chronograph

with tuning-fork arrangement—the so-called diapason. The tuning-fork is worked by an electro-magnet at an ascertained rate per second, and is continuous and regular. It is claimed that this instrument, if not altogether novel, is very accurate. The principle of the microphone has been utilized for certain instruments of diagnosis and experiment. As is well-known, the microphone is so made that the electric force is transmitted through two surfaces in loose contact with each other: in these instruments the substance used is carbon. The smallest vibrations are thus transmitted, and—as it were—amplified, to a telephone receiver. So delicate is the instrument that the footfalls of an insect are distinctly heard with it. Boudet seized upon this principle to explore the *audible* contractions of muscle, and we have thus displayed his myophone. This instrument, it is asserted, will give comparative notes to the ear from muscular tissue in different states of tonicity and degeneration. It has been used, we believe, in locomotor ataxia, but with what clinical results we cannot here state. We have also the cardiophone and laryngophone, whose names sufficiently indicate their purpose.

Another instrument exhibited is a dynamograph—a modified dynamometer, arranged to record on the sensitive sheet of a chronograph. There are also many forms of excitateurs for stimulating muscles and nerves with electric currents. These instruments are from Verdin, of Paris.

There are fine tympani for experiments on animals, and a patent anæsthetizer for dogs, and stretchers for dogs, cats, and rabbits, which, while not exactly electrical, are in appearance calculated to excite the interest of both vivisectionist and anti-vivisectionist.

A curious and interesting *phantom*, or model, of the brain on a large scale, is shown. It is constructed of wires, and shows the course of the fibres in the hemispheres, basal ganglia, and cord.

The electric light has found one of its most interesting uses, to physicians, in the small incandescent illuminators which are being used for lighting the cavities of the body during surgical and dental operations, and for illuminating the field of the microscope. There are several displays of these. Queen & Co. have a very cheap little glass globe, containing a carbon filament, which can be lighted by one Grenet cell, and seems admirably adapted to microscopic work. It sells for one dollar, although fittings, etc., considerably increase the expense. Another exhibitor, who styles himself an M. D., has a mysterious box which contains an inviolate secret: it is kept locked, the elements forever to be unknown to the professional world, and only the effect seen in a small globe of light, which is certainly very brilliant, and is claimed to be superior to any yet invented. The White Dental Manufacturing Co. also exhibit illuminators to which we will refer when we come to describe their interesting display of electrical dental apparatus.

There is nothing especially new at the Exhibition in batteries, unless we mention a somewhat modified Leclanché, called the Diamond battery. This is a simple zinc and carbon combination in a solution of ammonium chloride. There is no provision for depolarization, which is effected by time, the battery righting itself, as it were, in a few hours after use. Its chief merit is its cheapness.

A very attractive exhibit is the patent electric incubator. The query naturally arises—Can chickens be hatched by electricity? That is not the scheme. The old-fashioned temperature of 102°, which has been maintained by the setting hen from time immemorial, is not dispensed with. It is only maintained permanently by an electrical appliance, and the hen is dispensed with. The heat of the incubator, which is supplied by an oil lamp, acts upon a rubber rod and expands it. At 102° the expansion of the rod has started an electric clock-work gearing which opens a ventilator in the top of the incubator and turns down the lamp. On cooling the rod contracts, the reverse process occurs, and so the temperature is kept at about one point automatically. To the medical mind, which has recently observed in the medical journals pictures of the French *couvuse*—designed as an artificial nurse for young and feeble infants—the suggestion comes, Why not heat it and regulate it with electricity just as the patent incubator?

A small surgical engine is exhibited. It is worked by an electric motor, and drives a simple piston and cylinder which is intended to act as a pump for exhausting abscess cavities, etc. It impresses one as being rather a toy than a useful instrument.

In the annex are two small rooms devoted to models, historical apparatus, and bibliographical collections. Here are to be seen many things, both curious and instructive—especially the early models, and in some cases the instruments themselves, of Franklin, Henry, Morse, and Edison.

We are pleased to record that the managers have retired one, at least, of the electric quacks to whose presence we regretted to be compelled to call attention in our first notice. The blood-cleanser is no longer there. There remain still, however, some few erratic therapeutists whose wares do not add to the scientific character of the Exhibition.

REVIEWS.

ELEMENTS OF HUMAN PHYSIOLOGY. By HENRY POWER, M.B. Lond., F.R.C.S., Ophthalmic Surgeon to St. Bartholomew's Hospital, London; Examiner in the Board of Anatomy and Physiology, Royal College of Surgeons. 47 engravings, pp. 389. Philadelphia: H. C. Lea's Son & Co., 1884.

THIS little book, one of the series of five manuals for students of medicine which supplement each other in regard to the entire round of physiological science, is an admirable condensation of the larger work of Dr. Carpenter, edited by Mr. Power. It has been, however, carefully brought up to date by its accomplished author, and now presents the latest English exposition of its department in the subject, which is well worth reading carefully over by every practitioner who desires to keep himself fully informed in regard to the latest discoveries respecting the physiology of man. The others of the series, which form complementary volumes, are *The Elements of Histology*, by Prof. Klein; *Clinical Chemistry*, by Dr. Ralfe; *Physical Physiology*, by Dr. MacGregor-Robertson; and *Comparative Physiology and Anatomy*, by Prof. Bell. If, as Mr. Power suggests, these five little books are read together, the attentive

student can scarcely fail to acquire a sound basis for the future practice of his profession.

The forty-seven engravings with which the book is illustrated are not quite worthy of the text, and the number of mere diagrams, especially in the chapter upon generation and development, is proportionately great. Some fault might also be found with the scantiness of the index, which even in a student's hand-book should be full and explicit, but these imperfections dwindle into almost imperceptible blemishes when considered in conjunction with the real and practical value of the work.

YEAR-BOOKS OF MEDICAL PROGRESS. A YEAR-BOOK OF SURGERY FOR 1883. Edited by CHARLES H. KNIGHT, M.D. 8vo., pp. x. 197. New York and London: G. P. Putnam's Sons, 1884.

THIS book is intended to give a summary of the most important contributions to the literature of surgery during the past year. Its contents are arranged topically, and refer to a large number of operative methods, some of which are old and well established, some comparatively new, or newly attracting attention. In regard to both, the editor's selection seems to be very judicious, and his work to furnish an excellent supplement to the private references which most industrious surgeons may be supposed to keep. Not the least creditable part of this book is the introductory chapter, in which a very brief summary of its contents is given, and a truly judicial spirit shown in the comments on debatable questions of theory and practice in surgery. To prepare such a book as this as well as the editor has done, not only shows considerable skill, but confers a benefit on the reading world which is often missed by original and more pretentious works.

SOCIETY PROCEEDINGS.

MEDICAL SOCIETY OF VIRGINIA.

Fifteenth Annual Session, held at Rawley Springs, September 9, 10, and 11, 1884.

(Concluded from p. 328.)

(Specially reported for THE MEDICAL NEWS.)

SEPTEMBER 11TH—THIRD DAY. MORNING SESSION.

IN response to an official telegram sent on the preceding afternoon to the first President and Honorary Fellow of the Society, Dr. Robert S. Payne, of Lynchburg, who has recently been stricken with paralysis, expressing the sympathy of the Society, a telegraphic reply was read, showing a deep sense of appreciation of the consideration shown him by the Society.

DR. WM. W. PARKER, of Richmond, by letter, presented the following resolution on

FREE MEDICAL EDUCATION:

Resolved, That, in the opinion of this Society, the establishment of eleemosynary medical schools with the present limited course of study in the States would be ruinous to the profession and a calamity to the public; that in our State it would be the means of driving away the best pupils and the most talented young men, and in the end ruin our own excellent medical schools

whose success and permanency should rest chiefly upon the talents of their teachers, and not upon the treasury of the Commonwealth. Adopted.

DR. SAMUEL B. MORRISON, of Brownsburg, presented a memorial of Dr. J. Marion Sims, an Honorary Fellow of the Society, which was adopted by a rising vote.

DR. SAMUEL B. MORRISON, of Brownsburg, also presented the *Report on Advances in Obstetrics and Diseases of Women*.

DR. GEORGE T. HARRISON, of New York, presented by invitation a paper on the

MODERN TREATMENT OF WOUNDS.

He thought the credit was due to Leiter of having been the first to devise a rational method of treatment with a full consciousness of the nature of the problem he had to solve. These problems are two: first to keep a wound aseptic; and, secondly, when wounds are already infected, to make them again aseptic by the application of antiseptic measures.

The first problem to be solved generally presents itself in this form. What is the method of performing an aseptic operation? This is accomplished: 1. By attention to the operating-table. It should be free from blood and pus, well disinfected by a solution of corrosive sublimate (1 to 1000), and covered with a perfectly clean blanket and sheet or other clean covering.

2. The field of operation must be carefully cleansed. The day before the operation the patient should have a general bath. That portion of the surface of the body which is the seat of operative measures is cleansed with soap and water. If there is much sebaceous matter, spirits of turpentine or ether should be used. Hairs in the vicinity should be removed by the razor, and the surface then disinfected by a five per cent. solution of carbolic acid or corrosive sublimate (1 to 2000).

3. The surgeon and his assistants should have hands and arms aseptic.

4. Instruments should be made strictly aseptic.

5. Hemorrhage must be arrested by tying all bleeding vessels with catgut.

6. The method of cleansing the wound is of importance. Silk or catgut should be used, rendered aseptic, of course. Drainage is essential. Rubber drainage tubing, or Neuber's decalcified bone drainage-tubes should be used. The rubber should be boiled before use and kept in a five per cent. solution of carbolic acid, and before introduction should be powdered with iodoform. The dressing necessary to maintain the aseptic condition was then described. The original Lister dressing was described, and Volkmann's modification explained, the object of which was to produce a uniform compression on the wound and its vicinity. The permanent dressing used by Neuber was described, the object being to apply such a dressing as could be kept on the wound intact until the healing process was completed, and in that way all interference with the wound-rest is avoided. The recent modifications of treatment introduced by Neuber, by which he proposes to give up drainage for all recent wounds, were described. He effects this merely by avoiding the formation of coagula within the wound. He, therefore, applies compression from without, buried catgut sutures, depression sutures, and skin implantation. Proof was then adduced to show that the healing process is accom-

plished more certainly and in a shorter time by the antiseptic than by any other method, and that the accidental wound diseases are best avoided by its use.

DR. SAMUEL B. MORRISON thought it best to let wounds alone, so far as probing was concerned. He had seen and heard of many cases that made him regard the use of probes as dangerous—especially in cases of gunshot wounds. Do not run the finger or the probe into a wound that possibly involves a vital tissue.

DR. HUNTER MCGUIRE, of Richmond, had used the antiseptic dressing, time and again, to its minutest detail, and is satisfied that Lister's first recommendations added to the fatality of cases. Carbolic acid is itself an irritant when used as Lister originally recommended it, and causes inflammations that oftentimes have proved fatal—especially in abdominal surgery. He is glad that plan has been abandoned. Antiseptics are undoubtedly good in large overcrowded city hospitals—where even the walls reek with septic poison. But for country practitioners to go out with the idea that town antiseptics were essential would be to teach a mistake. He appealed to the country doctors present to answer whether or not they had bad results from the refusal to use *antiseptic* surgery. Their successes were marvellous—equalling, when skilful surgeons operated, the most satisfactory tables that "antiseptic surgeons" could present. Where the air is pure, uncontaminated by vegetable or animal decomposition or septic poison, ordinary cleanliness of hands, instruments, and wounds, is altogether sufficient.

DR. I. H. STONE, of Lincoln, Loudoun County, thanked Dr. McGuire for his practical remarks, which strongly confirmed him in opinions he had long entertained. He had often deemed it useless, in his country practice, to resort so carefully to the antiseptic methods adopted, perhaps, with propriety in city practice. He instanced some rare cases—one of wound opening the ankle-joint. In five or six days, by the simple use of cleanliness, the patient began to get well and later on recovered without an untoward symptom. Another case—a man with a history of a general mash-up in a railroad accident, resulting in several wounds—without other antiseptics than ordinary cleanliness, the patient got well in some thirty-five or forty days.

DR. MORRISON thought we could not be too careful in obstetric cases. He always uses antiseptics as a hand-wash for himself, and vaginal lavement for his patients, to prevent such diseases as puerperal fever, etc.

DR. JOHN NEFF, of Harrisburg, highly appreciated the remarks of Dr. McGuire as confirmatory of his own experience and observations. In cases of doubt as to septic exposure, he uses carbolized cotton as a dressing for wounds.

DR. J. E. CHANCELLOR, of Charlottesville, thought no remark used in the discussion should serve as a license for the non-use of antiseptics. There is no excuse for neglecting every possible means of saving life or hastening recovery. Especially are antiseptics valuable in lying-in cases.

DR. MCGUIRE added that he would not leave the Society under the impression that he does not think favorably of antiseptics when they are needed. All he meant to say on this subject was that antiseptics, being a fashionable refuge of the day, there were many cases not needing their use in country practice. In this very place, where the Society is now assembled, with all of

its mountain salubrity, he would not care for the details of antiseptics in the event he were called upon to perform an amputation or open a belly. The experience of the country doctors who had spoken and the many personal conversations he had with other excellent country practitioners sustained him in this saying. He was not speaking of lying-in cases. He had reference only to general surgical cases in his remarks.

DR. MORRISON referred to a case in which the Lister spray was used according to the great author's recommendations, and the resulting irritation of the carbolic acid killed the patient.

DR. L. LANKFORD, of Bowers, could not bring himself to think that antiseptics were of so much less value in country practice than in cities. His early education after graduation was in a city hospital where all the details of antiseptic dressing and treatment were carefully carried out. He had seen doctors get off their horses in country practice, with sweaty hands, and without respect to the wash-bowl, at once enter upon the performance of a capital operation. He was unprepared to say what percentage of deaths or successes resulted, as compared with the practice of those who followed out every suggestion of the antiseptic method. But he thinks the doctor who does as he has known some do—without the slightest apparent regard for ordinary cleanliness—is guilty of a criminal act, especially in case of a vaginal examination of a puerperal woman.

DR. JESSE EWELL, SR., of Hickory Grove, stated that he had invariably made it a habit of his fifty years of practice to use soap and water on his hands before entering upon the performance of any vaginal examination or surgical operation.

DR. HUNTER, of Frederick Co., during an epidemic of scarlet fever, had some obstetric cases. He always carried with him at that time a solution of carbolic acid in glycerine, which he used on his hands, and he did not have a puerperal disease of any kind.

DR. ALEX. HARRIS, of Jeffersonson, Va., presented a paper on

MEDICINAL PROPERTIES AND THERAPEUTIC APPLICATION OF [VIRGINIA] FAUQUIER WHITE SULPHUR WATER.

The elevation of the springs is about seven hundred feet above sea-level. The hygienic surroundings of the hotel are unsurpassed. The remarkable lightness of the water, which has a strong sulphurous taste and smell, enables patients to take double the quantity of it as compared with free-stone water. Experience has shown that it is primarily diuretic, diaphoretic or purgative, and secondarily alterative and tonic. The first perceptible impression is stimulant, causing in many cases a sense of fulness of the head, and, in some, headache. The key to the explanation of the wonderful therapeutics credited to this water is furnished by its analysis. The nearly three grains of phosphate of iron, lime, and magnesia per gallon, supplies the pabulum which the overworked brain and nervous system require.

EVENING SESSION.

DR. JOSEPH A. WHITE, of Richmond, offered a resolution looking to the

ORGANIZATION OF A TRI-STATE MEDICAL SOCIETY, representing Virginia, North Carolina, and West Virginia. Adopted.

DR. M. A. RUST, of Richmond, in a paper on

TYPHO-MALARIAL FEVER,

reviewed the history of the disease as it first made its home in this country. He held that typhus and typhoid fever are not interchangeable, that one does not produce the other. During all the epidemics of typhoid fever in Richmond, the type has been generally mild, and mixed with malarial infection. Hence, oftentimes it is cured by quinine. He narrated his experience with the recent epidemic in Richmond, which was popularly known as "the Richmond fever." He spoke of the thermometric curve peculiar to the disease. He laid great stress on the bacillus typhosus as an etiological factor. Sometimes purely local causes develop typhoid fever. As to treatment, he thinks, as a rule, the best medicine is no medicine. Cold sponging gives comfort. He uses bismuth when there is profuse diarrhoea. Spirits and beef tea are to be given when the heart's action becomes feeble, and the resisting power low. He uses for diet, boiled milk throughout the whole course of the disease. The prognosis is generally better if there be a stormy beginning.

DR. J. S. STONE, of Lincoln, Loudoun County, read a paper on the *Use of Massage and Electricity in the Treatment of Chronic Hysteria and Allied Nervous Diseases*.

RECTAL ETHERIZATION.

DR. WM. H. COGGESHALL, of Richmond, presented a full and complete report of fifty cases in which this method had been employed, most of them gleaned from contemporaneous medical literature, but a few occurring in his own practice. After a careful analysis of the cases detailed in the different medical journals of this country, he concluded by saying that in certain operations about the face and mouth, there seemed possibly to be a future favorable for this method of anaesthesia. By this method the period of excitement, usually occurring at the beginning of anaesthesia, is certainly materially lessened, and insensibility is induced in a more quiet manner. There is also less probability of vomiting. The great danger to be dreaded is the occurrence of bloody diarrhoea or dysenteric evacuations, due to inflammation of the mucous membrane of the lower bowel. He related the details of two fatal cases, in which death was undoubtedly due to this cause alone. He did not agree with those surgeons who thought this method a valuable addition to our present means of producing anaesthesia, as there is too much risk to life involved to counterbalance the small amount of benefit to be derived from it.

DR. HUNTER MCGUIRE read a paper on

INTESTINAL OBSTRUCTION,

which was specially devoted to diagnosis. As to treatment, he gave no purgative. The sheet-anchor is an opiate to quiet the peristalsis. It is unreasonable to expect such mechanical treatment as the administration of crude mercury to do good. If the symptoms indicate that the obstruction is removable by an operation, and if the obstruction can be at all definitely located, open the abdomen.

Drs. George T. Harrison, of New York City; J. M. Toner, of Washington; William Selden, of Norfolk;

and F. M. Robertson, of South Carolina, were unanimously

ELECTED HONORARY FELLOWS.

DR. RICHARD T. STYLE, of Richmond, presented

THE TREASURER'S REPORT,

which showed that \$1016 had been collected during the year, and \$757.45 were expended, leaving in the treasury \$258.55.

In the evening, the managers of Rawley Springs tendered the Society

A BANQUET,

which was greatly enjoyed by the members.

SEPTEMBER 12TH—FOURTH DAY.

MORNING SESSION.

The order of business was the discussion on *Malarial Fever*, which was opened by a paper from Dr. R. B. Stover, of Richmond, Va., which, owing to his unavoidable absence, was read by Dr. Upshur.

DR. JOHN N. UPSHUR followed with a paper on

MALARIAL FEVER AS IT EXISTED IN RICHMOND IN THE SPRING OF 1884.

It only partially responded to the influence of quinine. He thinks the name *typho-malarial fever* a misnomer. The cause of the Richmond fever was probably atmospheric. He does not think the cause rested in the drinking water of the city. The disease was mostly limited to young people. Besides, the same fever prevailed in other parts of the State. In many cases he noticed a coolness of the surface of the body as compared to the real temperature, and a further peculiarity was the prominence of the nervous symptoms. He reported several illustrative cases.

DR. WM. L. ROBINSON, of Danville, presented a paper on

TYPHO-MALARIAL FEVER PECULIAR TO DANVILLE.

He stated that nothing is known as to the bacteria of typhoid fever. Typho-malarial fever is something different from simple typhoid fever under the influence of malaria. Quinine does not affect typhoid fever; it cures typho-malarial fever, especially when given hypodermically. Milk diet is not good in the latter fever; it is generally rejected by the stomach. Beef tea, with its natural salts, is much better adapted to such cases. Typho-malarial fever does not prevail in the mountains as does typhoid fever.

DR. THOS. J. MOORE, of Richmond, Va., said that what malaria is has not been decided. It has no known limits of territory. We simply know that heat, moisture, and vegetable decomposition are essential factors. In wet sections of the country, we may do something by draining the marshes. He spoke especially of the two forms in which the disease generally manifests itself—the hemorrhagic and the pernicious. As to treatment, in all grave cases use quinine hypodermically. In the same way, Warburg's tincture may be administered, as he first saw recommended by Dr. Metcalf, of New York. The combination of morphine with the quinine is sometimes required in order to quiet the nervous perturbations.

DR. M. A. RUST, of Richmond, Va., remarked that

the views expressed in Dr. Upshur's paper are in perfect harmony with his own. The sooner we do away with the term typho-malarial fever the better. He would only comment on two points:

1st. Such expressions as "atmospheric influences," or "something in the air," as pathogenic causes are misleading.

2d. "Typhoid fever modified by malarial poison." This expression involves the supposition that the pathogenic germs of both typhoid and malarial fever have entered the human body, and produce, as allies or antagonists, a certain series of phenomena. We have no proofs whatever to support such a theory, and if we admit it, we might as well admit the term typho-malarial fever. But he had shown, in the paper read the evening before, that typhoid fever must necessarily present all grades and shades of severity, from zero to the maximum, and that slighter forms of typhoid fever, with a duration of a few days only, are frequently observed.

The subject of *Scarlet Fever* was selected for general discussion at the next annual session, and Dr. Thomas J. Moore, of Richmond, was elected to open it.

Dr. H. M. Clarkson, of Haymarket, was elected to deliver the next *Annual Address to the Public and Profession*.

Upon invitation, the Alleghany Springs, of Montgomery County, Va., two miles from the Norfolk and Western R. R., was selected as the place for the next meeting.

At 12 M. the Society adjourned.

RHODE ISLAND MEDICAL SOCIETY.

Quarterly Meeting, held at Providence September 18, 1884.

(Specially reported for THE MEDICAL NEWS.)

THE SOCIETY met in Lyceum Hall, Providence, THE PRESIDENT, DR. O. C. WIGGIN, in the Chair.

DR. GEO. D. HERSEY, Secretary, read the records of the annual meeting.

Upon the recommendation of the Board of Censors, Drs. Gonzolo E. Buxton and Frank A. Payan were elected to fellowship.

THE PRESIDENT'S ADDRESS.

THE PRESIDENT briefly addressed the Society, asking for a manifestation of greater literary and scientific interest on the part of the Fellows. Attention was called to the fact that of the *twenty-four* medical examiners recently appointed in Rhode Island, *twenty* are Fellows of this Society and that it now becomes the duty of these officials to instruct themselves thoroughly with reference to the responsibilities they will meet. This matter of self-instruction would not have been easily accomplished a few years ago, but now the library of the Society offers excellent facilities for such research, embracing, as it does, a complete set of the *Transactions* of the Massachusetts Medico-Legal Society, the publications of the Sydenham Society, and numerous monographs on kindred subjects.

He believed it would be well for the Society to have some special subject for investigation each year, and as most organized medical bodies are tending in the direction of sanitary science, he would suggest for this year an investigation of those measures which will offer the greatest security against a visitation of Asiatic cholera

to this country next year. He further remarked that whatever knowledge the public possess of correct living has come largely from the teaching of medical men. The habits of living in the majority of households are certainly better than they were a quarter of a century ago. The great advance in the matter of school hygiene was alluded to as one of the most important subjects physicians have ever sought to influence.

Contrary to the speaker's expectation, he had ascertained that in physical development American children are now superior to those of any other nationality.

It was finally urged that the Society establish a museum of pathological and anatomical specimens, to embrace illustrations of comparative as well as of human anatomy, both in health and disease.

REMOVAL OF A BROKEN CATHETER FROM THE URETHRA.

DR. S. O. MYERS, of Wickford, reported the following case: A man, sixty years of age, on account of partial paralysis, had for several years been resorting to self-catheterization, using a No. 10 American soft catheter. On grounds of economy, he did not renew the instrument except at long intervals. The last one used had become much elongated and very rotten, and had been partially broken in two and sewed together by the man's wife. Finally, when being withdrawn the instrument broke completely off, leaving some five inches of its length in the urethra and bladder. Dr. Myers found the end of the fragment about an inch anterior to the prostate gland, and succeeded in grasping it with the straight uterine forceps. It was so friable that the end grasped was twice broken off by the forceps, but the third attempt was successful and the remainder of the catheter was withdrawn.

DR. CASWELL remarked, in reply to a question by Dr. Myers, that probably the lithotrite is the best instrument with which to grasp a piece of soft catheter that is retained wholly within the bladder.

DR. CASWELL, of Providence, then reported

A CASE OF PELVIC ABSCESS.

the patient being 45 years old, stout and florid, and the mother of one child 17 years old. The woman had been subject to very severe attacks of metrorrhagia since the birth of her child, and it was thought advisable, last February, to dilate the os uteri and seek for the cause of the copious hemorrhages.

A sea-tangle tent was introduced, and followed by a second one, and that by a carbolized sponge, which completed the dilatation. Nothing abnormal was found within the uterus, except two enlarged Nabothian glands, to which he made an application of nitric acid. Toward the end of the first week general peritonitis developed, and was fully established by the tenth day subsequent to the dilatation. About five weeks later, a hard lump appeared in the left iliac region. Fluctuation was found ten weeks after the dilatation. An incision an inch and a half above Poupart's ligament gave exit to half a pint of healthy pus. From that time the case progressed very favorably, the patient being out of bed in two weeks, and entirely well at the end of a month. The excessive metrorrhagia has not yet returned. Dr. Caswell said that, without doubt, the abscess was the result of the use of the tents, and that the improvement in the chronic condition was due also to the tents, or to

the application of the acid. Although the woman's condition became a critical one, and for a long time it was impossible to discover the seat of the abscess, he should feel justified in employing the same mode of treatment in similar cases.

The same gentleman also briefly mentioned two other cases. In one an abscess developed in the middle portion of the abdominal cavity, and the pus forced its way through the intestinal wall into the bladder, as both pus and fecal matter were voided *per urethram*. The other case was one of pelvic abscess which opened into the urethra.

DR. J. W. PORTER remarked that there was comparatively little danger of causing pelvic abscess by the use of a *single* tent, but the *second* one is apt to cause the trouble. He regarded the sea-tangle tent as safer to use than those made of sponge. Furthermore, in the majority of cases of uterine disease, pelvic cellulitis exists either in its active or dormant type, and when it does exist the use of tents should be avoided. He also thought that after the introduction of a tent the patient should be as carefully cautioned and watched as in case of a capital operation, and that for a physician to introduce a tent at his office and then allow the woman to walk home, though only to the next door, was criminal practice.

Next there was a discussion upon

GENERAL SANITATION AND ITS BEARING ON A POSSIBLE VISITATION OF CHOLERA NEXT YEAR.

DR. C. V. CHAPIN spoke at considerable length and gave valuable suggestions upon the maintenance of a sanitary condition of the city, claiming that a pure soil, pure air, and pure water were of chief importance in the defence of a given locality against an epidemic of cholera. The belief was expressed that, practically, to quarantine a place amounts to nothing in keeping off the disease.

DR. E. M. SNOW remarked that cholera first visited Providence in 1832, coming by way of Quebec, Montreal, Albany, and New York City. Great alarm was felt in Providence at the time, and a rigid quarantine was kept up against New York. Physicians were sent to New York to investigate the subject, and on their return they were not allowed to approach the city until they had been fumigated at a remote point. The report they submitted was published, and three editions were exhausted in ten days, so great was the excitement. The city was thoroughly cleaned up and not a case appeared for a long time, but the disease finally appeared suddenly, and caused twenty-five deaths. It was noticed that the cases were widely distributed, a single death only occurring in a family or neighborhood.

The second visitation of cholera occurred in 1849, when there were 150 deaths out of about 225 cases. Of the decedents 51 were Americans and 99 were foreigners. A national fast was proclaimed at that time and strictly observed. It was also true that the disease increased its ravages immediately after that holiday.

In 1854 cholera caused 159 deaths in Providence. Dr. Snow said he had personally seen some 200 cases of cholera and had carefully examined each case. He had never thought it a contagious disease.

After further discussion upon matters of local sanitation, the Society adjourned to a collation at Ardoene's.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Montreal Meeting, September 1 and 2, 1884.

(Specially reported for THE MEDICAL NEWS.)

PHYSIOLOGICAL SUB-SECTION OF THE BIOLOGICAL SECTION.

THE physiologists, English and American, attending the meeting of the British Association with commendable enthusiasm formed a sub-section for communications of special interest to them. They met in the medical school building, and there was, in addition to the physiologists themselves, a very fair attendance; the medical profession of the city and from abroad being well represented by some of its ablest members.

The chair was taken by PROF. SCHÄFER, of University College, London, one of the vice-presidents of the Biological Section.

The first communication on the list was by DR. H. N. MARTIN, Director of the Biological Laboratory of the Johns Hopkins University, Baltimore, and MR. W. H. HOWELL, a Fellow of that institution, and was

ON THE COAGULATION OF BLOOD.

They had, from their experiments, concluded that blood taken from different parts of the slider terrapin's vascular system clotted in an equal length of time; though a contrary view had been published by another observer. Their results tended to confirm the conclusions of Hammarsten rather than of Schmidt, viz., that paraglobulin is not absolutely essential for coagulation. Turtle's blood was allowed to stand upon ice for twenty-four hours, two-thirds of the plasma siphoned off; treated with sodium chloride to saturation to get fibrinogen, which could be dissolved in dilute saline solution. The fibrin ferment was made in the usual way. This added to plasma gave a clot which was rather more soluble in sodium chloride than ordinary fibrin. The ferment used was free from paraglobulin.

In reply to questions from Prof. Schäfer, DR. MARTIN stated that no experiments had been made to test the influence which lecithin had on coagulation, but that white corpuscles had been added to plasma with the result of causing clotting. He believed either that they, or a watery extract of them, were necessary for the process.

Prof. Schäfer stated that if the (white) blood-corpuscles in the echinoderms were filtered off, coagulation did not take place.

THE BLOOD OF LIMULUS POLYPHEMUS,

by F. GOLCH and I. P. LAWS, of Oxford. The most interesting facts were that the serum was more like seawater than mammalian serum; the clot was more albuminous than fibrinous; that the coloring matter was allied to hæmocyannin; and that copper seemed to combine in this animal's blood with a proteid as iron does in the blood of mammals.

DR. MILLS asked if it appeared from the paper of the authors (who were themselves not present) whether they had taken special precautions in the ashing of the blood to avoid errors and fallacies as to chlorine, phosphoric acid, and carbonic dioxide.

In reply, PROF. SCHÄFER said that it did not appear from the paper itself, but he knew Mr. Laws to be an excellent chemist. The authors of the communication

had had a great abundance of the blood to work upon. They did not give spectroscopic appearances, about which both Dr. Mills and Dr. McKendrick had asked.

ON VASO-MOTOR NERVES,

DR. BOWDITCH, Professor of Physiology, Harvard University, remarked that he had not intended to read a paper, but he would give some account of work he had been engaged upon for the past year or two.

He would, for the sake of having a working physiological hypothesis, assume that there were perivascular vaso-motor ganglia and vaso-dilator and vaso-constrictor nerves. Instead of dealing with variations in blood pressure by the thermometric method, he has constructed an apparatus on the *plethysmographic* principle, but finding a spring not delicate enough, had made use of a column of compressed air, acting on a Marey's drum and being graphically represented on a cylinder with a very slow rotation, which was moved by a special clockwork. The curves due to changes in pressure were so delicately represented that variations due to cardiac beat and to respiration were represented. The animals were curarized. He had found that the curve obtained depended largely on the frequency of the stimulus, but he purposed to continue the work, which was not yet complete.

PROF. MARTIN referred to a phenomenon he had observed in connection with the quantity of fluid passing through the vessels of an animal deprived of the heart, and in which a sort of artificial heart was substituted—*i. e.*, an apparatus allowing of a pause equal in length to the time of action, and with the result that as much fluid passed through when the action of the pump was intermittent as when a constant flow was maintained by Mariotte's flasks. He offered no explanation of this.

PROF. SCHÄFER thought that if we adopted the view that the action of the muscular substance of the vessels varied according to the kind of stimulus, for which we have some evidence, we might get a simpler explanation than that of vaso-dilator and vaso-constrictor fibres. This might also throw some light on the phenomenon mentioned by Dr. Martin. He greatly admired the ingenuity of the apparatus and methods of Dr. Bowditch, as did all the members of the Section.

DEMONSTRATION OF THE COÖRDINATING HEART-CENTRE OF KRONECKER.

DR. MILLS, Demonstrator of Physiology, McGill University, Montreal, previous to the sitting, had given, after the method of Kronecker, a practical demonstration. A dog was narcotized with morphia; curarized; the thorax opened in the middle line to avoid bleeding; the pericardium opened and stitched back, and a needle thrust into the actively beating heart while artificial respiration was maintained. When successfully done, the ventricles go into fibrillar contraction, while the auricles beat as before. The latter are under the influence of the vagus, but not the former. This is due to injury to a centre which is not a motor centre, nor an inhibiting centre, but a *coördinating* centre. At present this cannot be more definitely located than by saying that it is situated toward the main branch of the left coronary artery, and at the lower border of the upper third of the ventricle. Prof. Kronecker believes this centre is that injured in

slight wounds of the heart causing death, in certain cases of sudden death from cardiac disease and typhoid fever, and in chloroform poisoning.

PROF. MARTIN had seen the phenomena when attempting to pass a canula into the coronary artery. He thought it was more likely due to the injury of one of the nerves near the coronary than to injury of a centre. He supposed that this interfered with the coördinated action of the muscles of the ventricle as brought about by the auricle.

PROF. SCHÄFER had seen the phenomenon by electric excitation of the ventricle, and took a view similar to Dr. Martin's.

PROF. BOWDITCH said it appeared that the injuries referred to by Dr. Martin were of nerves located on the surface, whereas in the demonstration it had been seen that the substance of the heart had been pierced.

PROF. MCKENDRICK, of Glasgow, thought that if it were due to injury of a nerve, recovery would take place in some instances at least, while in all these cases the dogs never had recovered the regular beat of the heart, as he had understood.

DR. MILLS thought the remarks of Prof. Bowditch and Prof. McKendrick were serious objections to the views of Prof. Martin and Prof. Schäfer. The theory of Prof. Kronecker was clear and simple, while it was doubtful if one like that advanced by Profs. Martin and Schäfer was even yet warranted by the work of Gaskell and others as to the relation of auricle and ventricle in the causation of the cardiac cycle, while it was possible to understand how injury to a superficial nerve might interfere with the action of the centre.

ON THE CARDIAC NERVES OF THE TURTLE,

by PROF. HUGO KRONECKER, of Berlin, and DR. MILLS, of Montreal. In 1880, Prof. Kronecker had found *accelerator* nerves in sea turtles which he had studied at Naples. He also noted the peculiar behavior of the *vagus*. These experiments the authors had repeated in the Berlin Physiological Laboratory. They found that the nerves (accelerators) varied in different species, and even in different individuals. They also observed in two species of land turtle that the *vena cava superiores* were under the influence of the vagus.

DR. MARTIN referred to a thoracic ganglion with a branch from it to the heart, with an accelerating function, in the slider terrapin.

ON THE FUNCTIONS OF THE MARGINAL CONVOLUTION,

PROF. SCHÄFER and MR. V. HORSLEY had undertaken experiments on the marginal convolution to endeavor to fill up the gap in the localization hypothesis of Ferrier, who found that electric stimulation had little, if any, effect on the muscles of the trunk. But these observers, by stimulating different small, well-localized areas of the marginal convolution of the monkey, did get movements of the muscles of the trunk, and the further forwards the stimulation, the more anterior the muscles concerned. By removal of these areas carefully on one side they had got incomplete paralysis; but when both sides were operated on there was such complete paralysis that the animal could not stand or sit; the spinal muscles were completely paralyzed.

By carefully removing the frontal lobes under antiseptic precautions, no inflammation followed, but the

animal showed no paralysis. This is in opposition to the work of Munk on this subject. The matter was referred to by Prof. Purser, of Dublin, Prof. McKendrick, Dr. King, of Hull, England, Dr. Bowditch, and Dr. Howard, of Montreal.

DR. MILLS said he could, from personal observation, bear testimony to the careful nature of the experiments of Goltz, the great opponent of the localization theories.

TUESDAY, SEPTEMBER 2D—SECOND DAY.

A programme of eleven papers was presented to the Sub-section for to-day, several of which, however, might, with more propriety, have been dealt with elsewhere, as they had very little physiological bearing.

AN APPARATUS FOR RECORDING CHANGES OF VOLUME.

PROF. SCHÄFER exhibited an apparatus filled with oil, into which a frog's heart, etc., could be placed, the changes in volume of which, during the beat, expressed themselves in the motions of a delicate piston, which could be recorded on a slowly revolving drum. The heart was fed, by means of a double canula, with a solution of dried ox blood.

All present agreed that this was a very neat and accurate apparatus. Prof. Schäfer stated that he had tested the accuracy of the cylinder's movements by photographing the movements of the column of oil.

REMARKS ON THE PROBLEM OF AQUATIC BREATHING,

PROF. MCKENDRICK, of Glasgow, described an apparatus which he had devised, and which he could apply to the gill covers of fishes, so as to register their motions. It consisted of a tambour, which, in some cases, was very small, fitted to the fish, to be observed and connected with the gill covers, the motions of which, acting on a column of air, were recorded on a revolving drum. By a swivel device the fish could move freely in the tank. The respiration curves were represented as found for a great many varieties of fish. They differed widely from one another, indicating much variety in the mode of respiration, but they agreed in that the more active fish, like the trout, had motions of the gills of a much more energetic kind than such a sluggish fish as the carp; but the amount of movement had no relation to the size of the fish. He had found the blood-pressure of fishes, in so far as examined, low—*e.g.*, in a pike twelve inches long, only 2 mm. of mercury. The tension of the gases in the blood of fishes, and also determination of hæmoglobin, had yet to be worked out.

PROF. MARTIN thought that the problem of the chemical energy liberated, owing to oxidation through respiration, etc., was simplified in the fish, inasmuch as it was not necessary that any of this energy should be converted into heat. He also suggested that some of the nudibranch amphibia of America might well be studied for the solution of kindred problems.

DR. MILLS mentioned that work on the respiration of fishes had lately been done in the Physiological Institute in Berlin, by a lady.

ON THE SECRETION OF OXALIC ACID IN THE DOG UNDER A VARYING DIET.

DR. MILLS said he had determined that of the methods extant, that of Schultzen was the best for the determination of oxalic acid in normal human urine, but in attempt-

ing to employ this method to dog's urine serious difficulties had been met with. After much labor he had so modified the method as to make it thoroughly applicable. The dog had had, after being brought into nitrogenous equilibrium, three diets, in succession, of (1) flesh, (2) flesh and fat, and (3) flesh and carbohydrates. He had shown that most oxalic acid was secreted under a flesh diet, which also rendered the urine highly acid; and more under a flesh and fat diet than on a diet of flesh and carbohydrates (bread). An elaborate table was shown, which brought together the results of the whole work which had extended over months.

PROF. SCHÄFER said no one who had not actually done such work could properly estimate the labor and care it involved.

DR. HOWARD, of Montreal, said that oxaluria was a very obscure disease, and it might be that this original work of Dr. Mills would throw some light on that subject.

ON THE MECHANISM OF ABSORPTION.

PROF. SCHÄFER, after referring to the difficulties of the mechanical theory of absorption, proceeded to explain that which for some years he had been inclined to hold and which he now believed partially demonstrated, though all the difficulties were not yet removed. Leucocytes had been observed between the columnar cells of the villi of the intestines, in the lymphoid tissue of the villus itself, and, later, in different degrees of disintegration in the lymphatic of the same. These had been fixed by osmic acid. His view was that these lymph-corpuscles, seen in such abundance, were the carriers of fat, etc., from the cells of the villus to the lymphatic; that they not only carried the digested food of the intestines, but assimilated it or further prepared it for the uses of the tissues, and then broke up and freed this material. There probably was also absorption direct by the columnar cells and the rest of the villus, but this was not the only way. He referred also to the absorption of the tail of the tadpole by the leucocytes migrated from the blood. His experiments outside the body to test the digestive capacity of the white blood-cells had been negative, but the conditions were very different from the natural ones.

PROF. MARTIN thought that while this theory did not do away with all the difficulties, it threw light on some points, such as explaining what became of the peptones which were not to be found in the blood to any appreciable degree, but were yet abundant in the digestive tract.

PROF. MCKENDRICK thought it in harmony with the later tendency to give more individuality to the different components of tissues.

DR. MINOT said that Zawarykin, unaware of Prof. Schäfer's work, had elaborated a similar theory.

DR. MILLS called Prof. Schäfer's attention to the fact that Landwehr, using his new and improved method of determining glycogen, could find no glycogen in leucocytes, though the quantity investigated was large. Mr. Schäfer had stated that the white cells changed starch into glycogen, and that he could see more of this in those cells that had been fed with starch than in the others.

MR. SCHÄFER could never see the network in the villi which Klein and Cohnheim referred to, and he also differed in some points from Zawarykin.

ON THE GROWTH OF CHILDREN.

DR. MINOT, in his paper, spoke of the measurements and standards of Quetelet, so generally used as "essentially incorrect." Very extended investigations had shown that the American was not only taller, but broader—in fact, a larger man than the Englishman. He referred to *phases of growth*, but maintained that life's whole career indicated a decline from the commencement of an independent existence. We were dying even while developing. He referred to Dr. Bowditch's elaborate measurements, etc., of the Boston school-children.

DR. MARTIN referred to an observation of Dr. Bowditch's that in a certain family, the members of which he was accustomed to weigh weekly, he had found that a falling off in the ratio of increase in the weight of the children often indicated latent disease, as an attack of whooping-cough, etc.

DR. MILLS thought it hard to overestimate the value of these laborious researches. He asked if his own impression that German women were heavier in proportion to height than American women was correct?

DR. MINOT replied that such was the case, though they were shorter than American women.

CLIMATE OF CANADA AND ITS RELATIONS TO LIFE AND HEALTH.

DR. W. H. HINGSTON, of Montreal, in this paper compared the French Canadian with the French of France; he was taller, heavier, and stronger than his French ancestor; the race was the "most prolific on the face of the earth," and the people did not seem to suffer by early marriages. The climate of Canada was singularly healthy, and there was no disease peculiar to the country, though climate modified the diseases common to Canada and other countries. There was an Anglo-American cast of countenance in the United States, but not in Canada. We are forming another type. The portraits he saw of the candidates for President strongly reminded him of the best specimens of the aborigines, barring color, beard, etc.

Several of the English members spoke on this subject, as well as some from the United States and Canada, very few seeming to agree with Dr. Hingston.

This closed the work of the sub-section for this year.

CORRESPONDENCE.

CHOLERA ON VESSELS AT SEA.

To the Editor of THE MEDICAL NEWS.

SIR: I must respectfully ask to be allowed to take exception to a remark in your editorial of September 6th, p. 267, on the "Cholera Conference at Berlin," as to some cases mentioned by me being "apocryphal," etc. On the contrary, those cases upon which I insist (one only being stated as given upon secondary information) were made certain by direct and careful inquiry at the time. Not satisfied with the account received from the captain of the packet-ship *Tonawanda* (which was attacked with cholera when two weeks out from Liverpool), I visited the medical officer who had been on the vessel during the voyage, and obtained from him a professional narrative of the occurrence described. This was in 1854, when, by the epidemics of 1848-49 and afterwards, in Europe and America, medi-

cal men had become sufficiently familiar with cholera to need to make no mistake in its diagnosis, by sea or land.

As to such cases being "always apocryphal," I must refer to some well-known authorities. Dr. James Wynne, in his elaborate "Report on Cholera in the United States," and Dr. Gavin Milroy (one of the most eminent sanitarians of Great Britain), in the *British and Foreign Medico-Chirurgical Review*, October, 1865 (p. 444), assert distinctly that in 1848 two vessels, the *New York* and the *Swanton*, were attacked with cholera—one when sixteen and the other twenty-seven days out from Havre, when there was no cholera at the latter port when they left it.

Let me cite from Dr. Koch himself, as reported in THE MEDICAL NEWS, Sept. 13, p. 294, a very close approach to a corroboration of this as a possibility. He observes: "It is noteworthy that on trading vessels, which carry all sorts of articles which could contain the infecting material of cholera, cholera never breaks out except during the first few days after leaving port. But on larger vessels it is otherwise; the outbreak may occur soon after leaving port, or it may not occur for two, three, or four weeks, or longer." No doubt Dr. Koch supposes such things to happen only when the vessels have left an infected port; but, besides the excessive improbability of such a latent culture of the bacilli as, on his theory, must be thus involved, we may repose upon the authority of Drs. Milroy and Wynne, against any such necessity, as above stated.

Many other facts, not at all apocryphal, could be easily brought up, were space afforded for them, in maintenance of the position I have taken. Such have been set forth in my essay on "Cholera, Facts and Conclusions,"¹ etc. Coming down to the present season, notice should be taken of the report to the Académie de Médecine of M. Brouardel, on his return from a mission of inquiry at Toulon; given in a letter from Paris, dated July 2d, and published in the *Weekly London Times* of July 4, 1884. Dr. Brouardel states that the first case of cholera at Toulon was that of a sailor on board of the *Montebello* (a vessel which had not left port for a long time), June 14th; the second case another sailor on the same vessel, June 15th. Neither of these men had been to sea for four or five years, and had no communication with the town or with the rest of the fleet. As to the *Sarthe*, a vessel which was accused of bringing the cholera from China, although there appear to have been two cases on board at Saigon, China, April 1st and 2d, the patients had been sent ashore, their beds disinfected, and the ship emptied, scraped, and well fumigated; while, during the *forty-five days'* passage to Toulon, no case had occurred in her. M. Brouardel very safely declares, therefore, that the *Sarthe* could not have brought the infection; and the *Montebello* sailors were shown not to have had the slightest communication with her sailors or cargo.

At the present date it may be fairly permissible to add another question to those propounded at the recent Berlin Conference: *What has become of the cholera-producing comma-bacilli at Toulon and Marseilles* where almost no more cases occur, at a time when (Sept. 15, 1884) over twelve hundred cases are reported in one day at Naples, with half as many deaths?

¹ Philadelphia: J. B. Lippincott & Co., 1866.

In conclusion, I beg to say that when such facts as those upon which I have insisted are omitted from a discussion of their subject, it is simply so much the worse for the scientific character of the discussion. And the same remark applies to the share of attention given to or withheld from the very careful and minute investigations of Strauss and Roux in Egypt and Toulon, as reported in *THE MEDICAL NEWS*, August 30th, p. 237, adverse to Dr. Koch's results.

HENRY HARTSHORNE, M.D.

PHILADELPHIA, September 15, 1884.

NEW INVENTIONS.

A POCKET URINARY TEST-CASE.

We have received from Parke, Davis & Co., a very neat and convenient pocket urinary test-case, which we think will become a favorite with the profession. It contains a complete set of Dr. Oliver's urinary test-papers for albumen and sugar, enclosed in an envelope, on which are printed directions for their use; also a rubber-capped dropper and two test-tubes, of which one is appropriately graduated.

We do not think that bedside urinary testing will ever be as popular in this country as it seems to be in England, but for the physician or consultant who has to visit a patient at a distance, this test-case is invaluable, far surpassing in convenience and security anything which has yet been suggested.

NEWS ITEMS.

THE AMERICAN PUBLIC HEALTH ASSOCIATION begins its eleventh annual meeting at St. Louis, October 14th, and continues four days. The subjects designated by the committee for special consideration are: (1) Hygiene of the Habitations of the Poor, (2) Hygiene of Occupations, (3) School Hygiene, (4) Adulteration of Food, (5) Water Pollution, (6) Disposal of Sewerage by Chemical Action, (7) The Observable Effects upon Public Health of Official Supervision, (8) The Work of Municipal and State Boards of Health.

It is believed that the meeting will be one of the largest ever held. The Conference of Charities and Corrections meets at St. Louis the same week.

THE LAST THING IN CHOLERA-GERMS.—The *British Medical Journal*, of September 6, states that Drs. S. MAURIN and LAUGE, who have been working at Marseilles since the departure of the German Commission, report that they have found a mucor, which they believe to be the actual agent in the propagation of cholera, and which is the mature form of the bacillus in an earlier and lower phase. This mucor appears, they say, on the fourth or fifth day, on the putrefying stools of cholera, and on these only. It has the form of a mycelium, the tapering ends of which are surmounted by cup-shaped sporangia, which burst on the slightest agitation, discharging vast number of spores. These spores require for their germination to be deposited on, or to come in contact with, some putrid organic matter, when they develop into a mucor of another form, an anaërobium, which they believe to be the immediate cause of the phenomena of the disease, and which, in its turn sporifying, gives birth to the bacilli of Koch.

The bacilli themselves are innocuous, but deposited on a putrid medium, and in contact with the air, they develop the first-mentioned mucor; and the cycle is renewed.

The first mucor, unlike the bacillus, possesses in a high degree the power of resisting the action of the so-called disinfectants; it is not killed by ten per cent. solutions of nitric or hydrochloric acid; it vegetates freely in a solution of carbolic acid of the same strength, and can sustain any temperature up to 302° Fah., but above this it breaks up, as it does also in a ten per cent. solution of tincture of iodine; and a specimen mounted in oil of turpentine went through its whole development up to the discharge of its spores. The bearing of these statements on the causation of the spread of cholera makes their verification a matter of the highest importance.

KOCH'S METHOD OF CULTIVATING COMMA-BACILLI.—The Berlin correspondent of the *British Medical Journal* writes that, since the publication of Koch's Address, it was suggested that it would be advisable to give medical men the opportunity of knowing something more about the investigation of bacteria. Herr von Gossler, Minister of Public Worship, Education, and Medical Affairs, has ordered that a certain number of medical men be summoned to Berlin every year, to go through a course of study, lasting from two to three weeks, in order to learn the new methods of investigation connected with bacteria and microorganisms, but chiefly to become acquainted with everything connected with the comma-bacillus and the methods of cultivating it, used by Koch. The several federal governments have been already requested to select a number of medical men for this course. The day for the commencement of the first course will be fixed shortly.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM SEPTEMBER 16 TO SEPTEMBER 22, 1884.

CALDWELL, D. G., *Major and Surgeon*.—Granted leave of absence for one month and twenty days, to commence about October 15, 1884.—*S. O. 95, Headquarters Division of the Missouri, August 16, 1884.*

MIDDLETON, PASSMORE, *Major and Surgeon*.—Having reported from sick leave of absence, assigned to duty at Fort Leavenworth, Kansas.—*Par. 2, S. O. 188, Headquarters Department of Missouri, September 19, 1884.*

CRONKLISTE, HENRY M., *Captain and Assistant Surgeon*.—From Department of the Platte to Department of the Missouri.

TAYLOR, ARTHUR W., *First Lieutenant and Assistant Surgeon*.—From Department of the Missouri to Department of the Platte.—*Par. 1, S. O. 215, A. G. O., September 13, 1884.*

WHITE, ROBERT H., *Captain and Assistant Surgeon*.—Ordered for duty as Post Surgeon, Fort Gaston, California.—*Par. 2, S. O. 110, Headquarters Department of California, September 13, 1884.*

WILSON, WM. J., *Captain and Assistant Surgeon*.—From Department of Dakota to Department of the East.

GARDINER, J. DE B. W., *Captain and Assistant Surgeon*.—From Department of Arizona to Department of the East.

CORBUSIER, WM. H., *Captain and Assistant Surgeon*.—From Department of East to Department of the Arizona.

LA GARDE, L. A., *Captain and Assistant Surgeon*.—From Department of Missouri to Department of the Dakota.—*Par. 1, S. O. 220, A. G. O., September 19, 1884.*

BARROWS, C. C., *First Lieutenant and Assistant Surgeon*.—Granted one month's leave of absence with permission to apply to the proper authority for an extension of one month.—*Par. 4, S. O. 86, Headquarters Department of Arizona, September 13, 1884.*